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American Political Science Review

Table of Contents

ARTICLES

Issues, Candidate Image, and Priming: The Use of Private Polls in Kennedy's 1960 Presidential Campaign
Lawrence R. Jacobs and Robert Y. Shapiro 527

Enhancing Democracy through Legislative Redistricting
Andrew Gelman and Gary King 541

Power and Influence in State Legislative Policymaking: The Interaction of Gender and Position in Committee Hearing Debates
Lyn Kathlene 560

Domestic Political Audiences and the Escalation of International Disputes
James D. Fearon 577

Peace, War, and the Balance of Power
R. Harrison Wagner 593

Strategic Voting Equilibria under the Single Nontransferable Vote
Gary W. Cox 608

The Tower of Babel as a Coordination Game: Political Linguistics in Ghana
David D. Laitin 622

Civic Culture and Democracy: The Question of Causal Relationships
Edward N. Muller and Mitchell A. Seligson 635

On the Idea of the Moral Economy
William James Booth 653

The Piety of Thought in Plato's *Republic*, Book I
Darrell Dobbs 668

Thoughtless Assertion and Political Deliberation
Melissa A. Orlie 684

RESEARCH NOTE

An Individual-level Multiequation Model of Expenditure Effects in Contested House Elections
Christopher Kenny and Michael McBurnett 699

CONTROVERSY

Popular Influence on Supreme Court Decisions
Helmut Norpoth and Jeffrey A. Segal;
William Mishler and Reginald S. Sheehan 711

NOTES FROM THE MANAGING EDITOR 725

BOOK REVIEWS

Political Theory 732
American Politics 751
Comparative Poltiics 776
International Relations 793

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ARTICLES

American Political Science Review Vol. 88, No. 3 September 1994

ISSUES, CANDIDATE IMAGE, AND PRIMING: THE USE OF PRIVATE POLLS IN KENNEDY'S 1960 PRESIDENTIAL CAMPAIGN

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Interpretations of electoral campaigns have pointed to two mutually exclusive strategies: candidates are expected to focus either on policy issues or on personal image. We argue, however, that social psychologists' notion of priming *offers an empirically grounded and theoretically plausible campaign strategy for treating image and issues as interconnected strategic concerns. Based on both quantitative and historical analysis of John F. Kennedy's 1960 presidential campaign, we find that the candidate's policy positions were related to results from his private public opinion polls. Archival and interview evidence suggests that Kennedy deliberately used these popular issues to shape the electorate's standards for evaluating his personal attributes (rather than to win over utility-maximizing voters). We conclude that the study of priming offers one important approach to reintegrating research on candidate strategy and voter behavior.*

Analysis of election campaigns typically assumes that rational candidates avoid taking divisive issue positions and instead craft ambiguous policy stances and attractive personal images of strength, boldness, competence, honesty, and trustworthiness (see Nie, Verba, and Petrocik 1979; Page 1978). Projecting favorable personal attributes and images is believed to offer a risk-averse strategy for holding together the party base and attracting wavering voters (Sniderman, Glaser, and Griffin 1990).

This conventional wisdom developed in a reaction against the argument—often associated with the work of Anthony Downs (1957)—that officeholders should appeal to a self-interested electorate by concentrating on policy issues and, specifically, on minimizing the distance between their distinct issue positions and voters' policy preferences.[1] Research by the personal attribute school has identified at least four reasons why rational candidates are expected to refrain from issue-based strategies: (1) the existence of multiple issue dimensions defies any office seeker's attempt to locate a position that will appeal to the median voter in all directions (but see Davis, Hinich, and Ordeshook 1970); (2) candidates are uncertain about their information concerning the electorate's preferences and their own ideal positions in relation to those preferences (Erikson and Romero 1990); (3) in this environment of uncertainty, candidates that announce definitive policy positions on conflictual issues risk alienating centrist voters; and (4) candidates are constrained in their position taking by ideologically oriented activists within the political parties (Kessel 1992; Page 1978).

However, empirical research, as well as recent economic and psychological theorizing suggest that candidates do not focus on developing a favorable personal image to the exclusion of position taking: policy stances and a candidate's personal image are not mutually exclusive bases for campaign strategy.

We draw on the social psychological process of priming as a way to understand the interconnectedness of image and issues in campaign strategy: this process suggests that candidates use popular policy issues to influence (or to "prime") the electorate's standards for evaluating their personal attributes. In particular, we focus on the case of John F. Kennedy's 1960 campaign and suggest that Louis Harris's innovative public opinion surveys heightened the Kennedy campaign's interest and skill in using position taking to shape the candidate's image. Harris's polls, which were privately conducted for Kennedy, offset a major deterrent of candidate position taking—limited information about voters' policy preferences. Our purpose is to use Kennedy's campaign to outline a candidate strategy that incorporates the public's policy preferences without ignoring the well established limitations on issue-based voting.

THE INTERCONNECTEDNESS OF ISSUES AND IMAGES

The study of campaigns can usefully draw on social psychologists' analysis of individual perception and, especially, on their distinction between the *availability* and the *accessibility* of public attitudes and information. This analysis holds that individuals have a set of enduring attitudes stored in their memories. Whether these attitudes are influential when a judgment must be made is determined by whether and how they are accessed or retrieved from memory (see Aldrich, Sullivan, and Borgida 1989; Iyengar 1990; Iyengar and Kinder 1987; Krosnick and Kinder 1990; Lavine et al. 1992). While some attitudes are frequently and consistently activated from memory, others are temporarily accessed because of immediate environmental stimuli.

Research has typically focused on the mass media because it provides individuals with much of their information and therefore may be a primary stimulus for accessing attitudes. Indeed, studies of individuals have found that emphasis on a particular policy issue in the media may "prime" or temporarily increase the accessibility of these attitudes.[2] For instance, as media coverage and discussion of health care reform increases the salience of the issue, the greater will be the probability that an individual's attitudes toward that issue (and perhaps related policies) will be activated and will become the standard by which he or she evaluates other attitude objects like candidates or related issues.

The process of priming, then, does not depend on altering an individual's policy preferences (e.g. converting supporters of health reform into opponents). Indeed, changing the public's policy preferences (especially during such short periods as an election campaign) is quite unlikely (see Campbell et al. 1960; Finkel 1993; Page and Shapiro 1992). Rather, priming involves a twofold process: (1) it sets the agenda by focusing public attention on certain topics, and (2) it provides the main basis for evaluation. Thus the messages communicated through and by the media during an election significantly influence which attitudes and information are likely to be retrieved or accessed from memory and incorporated into voters' judgments about, and ultimate choices of, candidates.

We propose to extend the application of priming from its original focus (the analysis of how individuals form attitudes and make decisions) to the study of candidate behavior. This new use changes the analytic focus from unintentional priming (i.e., the inadvertent impact of journalistic practices on voters) to *intentional* priming, namely, the deliberate strategies that candidates pursue to influence voters.[3]

The study of priming contributes to understanding candidate behavior by addressing three analytic obstacles to incorporating policy issues into campaign strategy: multiple issue dimensions, incomplete or imperfect information, and image-oriented campaign strategy.

First, research in the fields of psychology and political communication suggests that priming largely offsets the problem of multiple issue dimensions. Previous studies report that the media's attention to a specific issue activates existing attitudes on that policy or closely related attitudes on different policies; the effect is to crowd out other issues (Iyengar 1990; Iyengar and Kinder 1987). Ample studies of issue voting and campaign advertising confirm that focusing voters' attention on a few particular issues influences the importance that the electorate as a whole attaches to them (Aldrich and Alvarez 1992; Aldrich, Sullivan, and Borgida 1989; Berelson, Lazarsfeld, and McPhee 1954; Finkel 1993; Geer 1992; Nie, Verba, and Petrocik 1979; Petrocik 1991; Popkin 1991; West 1991).[4]

A priming strategy is compatible, then, with a simpler and more realistic model of issue voting: when the electorate evaluates candidates, it neither possesses full information nor makes full use of the multidimensional space of issues. Rather, some issues are more important than others because priming prompts voters to gather readily available information on a few issues and to access a small subset of attitudes. The implication for campaign strategy is that candidates need not adopt issue positions that appeal in all directions: candidate appeals that activate existing attitudes on one policy issue suffocate or distract interest in other issues. In other words, campaign strategy involves the manageable task of influencing the weights voters attach to a few issues.

The second major obstacle to incorporating policy issues into campaign strategy is the risk associated with position taking in an environment of incomplete or imperfect information. One fundamental difference in candidate priming as opposed to media priming is that campaigns intentionally develop strategies to minimize risk and maximize benefits. The campaign's ability to prime voters to access stored information is constrained by several strategic (as well as real-world) considerations, including the risk that a candidate's position will generate new information and messages that alienate centrist voters.

Public opinion surveys, however, have bolstered candidates' confidence and sophistication in pursuing a priming strategy; the availability of measures of voters' concerns have mitigated candidate uncertainty over voters preferences and lowered the risk in basing a campaign on policy issues (Geer 1991). While candidates and presidents since Franklin Roosevelt have used scientific opinion surveys, John F. Kennedy's 1960 campaign introduced major innovations in terms of the number, structure, and political use of opinion polls (For the campaign's interest in simulation analysis, see Jacobs 1992b, 1993; Pool, Abelson, and Popkin 1964.)[5] Interviews and archival evidence suggest that the Kennedy campaign commissioned and regularly used private, state-level opinion surveys conducted by Louis Harris. The polls included responses to an open-ended question regarding the most important problem facing the country.[6] Kennedy's private surveys of the public's policy goals were ideally suited to tracking the salience and accessibility of policy issues. Indeed, responses to the "most important problem" question are widely used in contemporary analysis of voters and priming's impact on them (e.g., Abramson, Aldrich, and Rohde 1991, chap. 6; Aldrich, Sullivan, and Borgida 1989).

Campaign records and interviews indicate that the Kennedy campaign sought to diminish its uncertainty regarding voters' preferences by adopting two innovations proposed by Harris: to poll in separate states and forgo national surveys and to avoid "separat[ing] our polling from the other problems of strategy."[7] The political task of winning primaries and electoral college votes was closely related to polling that would, Harris explained to Kennedy, "tell you how . . . [a] critical state can be won."[8] Harris's position within Kennedy's inner circle of advisers insured that polling information was incorporated into the campaign's strategy.[9] In short, the Kennedy campaign committed scarce resources to

Harris's polls because they provided strategically oriented information on the policy issues that were salient and accessible to the average voter. The implication of Kennedy's campaign is that the priming concept is consistent with the information available to modern campaign organizations: extensive and sophisticated polling lowers a campaign's risk and enhances its skill in taking issue positions.

The third analytic payoff is that priming offers a theoretical framework for treating policy issues and candidate image as interconnected (rather than as conceptually distinct). When following a priming strategy, campaigns adopt a limited set of spatially distinct and popular issue positions. These policy stances are used by the campaign not as the exclusive basis of its strategy but as an instrument for emphasizing and thereby influencing the *standards* for evaluating a candidate's personal characteristics. A candidate's decision, then, to advocate a few popular issues adjusts the standards by which their leadership qualities will be perceived, evaluated, and judged. The strategic objective is to modify the public's evaluation of the candidate's image in order to widen the candidate's appeal.

A priming strategy does not require candidates to advocate positions the candidate actually opposes. Rather, it suggests that campaigns select a relatively few issues out of a large pool of acceptable possibilities for particular emphasis. In other words, candidate strategy involves strategic adjustments: highlighting a few policies by increasing the frequency, strength, and extensiveness of the candidate's statements.

Research confirms that campaigns face compelling reasons to combine strategic position taking with image building. A singular focus on policy issues runs into the well-known limitations of a strict Downsian strategy. But an exclusive focus on image is likely to frustrate voters who are quite interested in salient issues; the electorate's perception of candidate image does not occur separate from, or at the expense of, its policy preferences. Empirical studies have found that voters' perception of candidate competence or trustworthiness is associated with their policy preferences (Kessel 1992; Page 1978, chap. 8; Petrocik 1991; Popkin 1991; Rahn et al. 1990; Sniderman et al. 1990). In addition, recent models of campaign strategy suggest that when office seekers announce their positions, they "signal" or increase the perception of favorable nonpolicy characteristics (Banks 1991; Cameron and Enelow 1992). Priming, then, suggests an empirically grounded and theoretically plausible campaign strategy for integrating the public's policy preferences with the well-established limitations on issue-based voting.

We use John Kennedy's 1960 campaign to examine candidate strategy and behavior in modern presidential elections. We ask, Does the direct study of Kennedy's strategy suggest that the campaign depended exclusively either on the spatial positioning of its policy stances or on the shaping of the candidate's image, or did Kennedy (as we suspect) attach strategic advantage to using issues to activate or prime voters' policy attitudes?

If priming is a useful way to understand Kennedy's 1960 strategy, his campaign should exhibit behavior consistent with *two* theoretical expectations. First, to cut down on uncertainty concerning voters' positions on policy issues, the candidate should *respond* to reported polling data indicating which particular policy issues were highly salient and accessible. In other words, the specific policy areas and policy directions that Kennedy advocated should correspond with polling information regarding what most concerned Americans. Kennedy is expected to follow public opinion by making tactical adjustments both on which specific issues to support (out of a large pool of acceptable choices) and on the frequency and strength of his statements promoting a particular issue. This theoretical expectation contradicts the conventional wisdom that the importance of the public's policy preferences (as conceptualized in spatial terms) is severely mitigated by the multidimensionality of the issue space, the uncertainty surrounding the interpretation of public preferences, and the pressure of party activists.

Second, if Kennedy's position taking did respond to reported opinion, we would expect archival and interview evidence to indicate that his stances were not premised on winning over (in straightforward fashion) the Downsian utility-maximizing voter. Rather, the campaign's positions should have been developed to selectively prime voters in order to influence the electorate's standards for evaluating and judging competing office seekers. We would anticipate that the Kennedy campaign used salient, accessible issues in order to construct an appealing image of the candidate as competent and caring, an image that diverged quite noticeably (and favorably) from that of his opponent.

METHODS AND MEASURES

Our analysis of Kennedy's campaign behavior in 1960 is based on primary evidence drawn from archival records and interviews, as well as on a combination of interpretive and quantitative analysis. Typically, campaign analysis has relied either on inferences regarding statistical relationships between candidate behavior and public opinion or on formal theoretical models in order to speculate about expected strategies. However, questions concerning how and why real politicians use and are influenced by polling results have been neglected.

To incorporate the political motivations and calculations of individual office seekers, we analyze the Kennedy campaign's archival records, along with interviews with key participants. These sources provide intensive, in-depth evidence to explore, through a few specific policies such as health care reform, the extent to which polling results cause a campaign to advocate specific policies. Moreover, archival evidence and interviews provide the basis for investigating the focus of our second theoretical expectation, namely, the campaign's strategic intentions and calculations for using opinion polls.

We supplement this interpretive study of archival records and interviews with quantitative analysis of the relationship between public opinion and Kennedy's policy positions. In particular, quantitative analysis enables us to investigate further the extent of Kennedy's responsiveness to reported public opinion.

For the statistical analysis, we collected data on public opinion and Kennedy's policy positions. Our public opinion data are new and better than those used in previous studies of candidate behavior. Past research on the public's impact on candidate behavior has relied on polling results that were published and widely circulated (e.g., Page 1978; Young 1991). The critical assumption has been that the published survey results found their way into the campaign's decision-making process (or at least reflected comparable information that was used by the campaign).

The question whether relying on published surveys is fully appropriate has been generally downplayed, given the common problem of identifying available opinion data. Clearly, however, polling results assembled and actually used by a candidate are precisely what is required for analyzing how far candidate decisions were influenced by public opinion. As far as we know, ours is the first to study systematically campaign data assembled by and for a major presidential candidate. In particular, Harris was an "integral part" of the campaign organization and regularly reported his polling results to Kennedy through both written accounts and frequent meetings with the candidate.[10]

Specifically, we use the policy goals that the public cited as important in Louis Harris's surveys for the Kennedy campaign; both campaign documents and our interview with Harris indicate that this body of data had "great importance" in the campaign. Harris's polls were based on personal interviews with large representative samples in separate states. Many of the states were polled on multiple occasions. According to records in the Kennedy Presidential Library, Harris conducted 66 polls during the primary and general election campaign, 26 of them between September and November 1960. Our analysis of Kennedy's campaign against Nixon focuses on the 26 opinion surveys conducted during the general election. Rather than being evenly distributed, these polls were bunched into six weeks: they were completed during the first half of September, the second week of October, and the contest's final weeks.

We studied Kennedy's most consistent source of data on public opinion toward policy issues, namely, Harris's "most important problem" question. These data have two important characteristics: (1) they measures the salience of an issue, telling us how a particular policy area (e.g., Social Security) was ranked compared to other issues; (2) because Harris structured his surveys to ask respondents, "What do you think can be done about [a national problem]," his polls also tracked the public's preferred policy *direction* (e.g., *increasing* Social Security payments).[11] (Racial integration offers another illustration of how issues were defined by Harris: he reported "pro-integration" as one issue and "anti-integration" as a separate issue.) The unique feature of Harris's "most important problem" question, as compared to the typical salience measures, is that it reports directionality. Harris's data include respondents' judgments about direction and salience. These polling results are well suited for our study of candidate priming precisely because they identify the specific issues and policy directions that were salient and accessible to voters.

Using Harris's polling reports to Kennedy, we constructed weekly opinion measures by averaging each state's poll results for weekly intervals. For instance, for the five separate state surveys that Harris conducted during the week of 23 October, we averaged the percentage of respondents who ranked increasing Social Security as an important problem.[12]

Data on Kennedy's position taking were obtained by content-analyzing two sources of Kennedy's publicly stated positions. One source of Kennedy's statements was the four debates with Vice President Nixon.[13] We treated each debate as a discrete event (i.e., we analyzed four distinct sets of policy statements). The debates seem to warrant this separate attention because of the importance that Kennedy (and history) attached to these first televised exchanges between presidential candidates. Moreover, polling results were, according to Harris and others, an important consideration in briefing Kennedy and plotting the campaign's strategy toward the debates.[14] The debates, however, are not a direct measure of Kennedy's strategic position taking: because journalists asked the questions and therefore controlled the agenda, the debates are at best an indirect, mediated avenue for activating voters' attitude structures.

A better and more direct indicator of Kennedy's statements is contained in a comprehensive collection of the candidates' day-to-day speeches and other statements, which was compiled by the U.S. Senate.[15] The Senate studies offer a record of the public statements of both candidates during the entire general election campaign.

Kennedy's policy stances in the debates and in his other public statements during the entire campaign were coded on a five-point scale from explicitly supportive (+2) to explicitly opposed (−2), with the midpoint (0) representing a neutral or unclear position (+1 and −1 indicate probable support and opposition, respectively).[16] We then summed these results to produce weekly measures for the content of Kennedy's statements on an issue.[17]

Our data, then, consist of 55 separate policy issues that were cited in Harris's poll reports and in Kennedy's public policy statements during the entire campaign, each issue being defined in a directional sense (e.g., increasing Social Security).[18] Because we collected data on 55 separate issues, we are able to conduct separate analyses of specific issues and policy directions. Our analysis is not confined to studying such broad policy categories as "social welfare" or foreign policy.

For each of the 55 issues, we created weekly measures of Kennedy's reported opinion poll results and his policy statements (as recorded by the Senate study) for each of the 10 weeks between 1 September and 8 November. The number of cases, then, is the sum total of both policy stances on, and polling data for, each of the 55 issues during the 10 weeks of the campaign, the maximum number being 550. Because Harris's opinion surveys were clustered during six weeks, however, the effective number of "issue-week" cases falls below this total. Technically, then, we have 55 (abbreviated) pooled time series, making it possible to compare many issues and time periods.

We created the weekly measures in order to reflect the campaign's operations and use of Harris's surveys. When it came to incorporating polling results, the campaign did not view each day as a discrete unit. Instead, Harris's surveys were conducted in clusters and the campaign drew on these blocks of results until the next batch was forwarded. Weekly aggregation of the opinion and policy data approximates this process of bundling together intermittent polling results in order to evaluate campaign strategy.

Our concern to capture the campaign's operations also led us to use time lags in analyzing the debates and the candidate's public statements (as compiled by the Senate). We dated Harris's results according to the completion of the surveys. Obviously, it took significant time both for Harris to enter and analyze the survey results and for the campaign to consider altering the candidate's speeches and other statements. According to campaign documents, Harris's data on the public's policy goals took on average two weeks to reach Kennedy for much of the campaign. For instance, we related Harris's results during the campaign's first week to Kennedy's public statements during the third week of September. For the campaign's closing week, however, the evidence suggests that a one-week lag is more appropriate: Harris shortened the report time as the urgency and demand for results increased.[19] (Indeed, more than a quarter of Harris's polls were completed with less than two weeks remaining in the campaign; these results were obviously used in much less than 14 days, and the last polls within a week.) The substantive reasons for using two-week lags during the campaign's first six weeks and one-week lags during its final four weeks is supported by our results; we examined a variety of lag structures and the proposed lag structure produced the clearest and strongest pattern of results.[20]

ISSUES, CANDIDATE IMAGE, AND CAMPAIGN STRATEGY

Interviews and archival records suggest that the Kennedy campaign analyzed the candidate's policy positions and the public's perception of his personal image as interconnected (rather than mutually exclusive) strategic concerns. Indeed, soon after the 1956 election, Kennedy was wondering, "What am I going to be for?" By the time the campaign gathered steam in 1959, his clear identification with a few policy positions (that were acceptable to the candidate) was seen as a way to adjust "the so-called Kennedy image."[21] Throughout the primary and general election campaigns, Kennedy's aides carefully tracked their candidate's image and attempted to pinpoint his perceived personal characteristics that were considered unfavorable. (Harris devoted a separate section of his surveys to tracking and analyzing the public's image of Kennedy's personality and his job performance.) Thus, when aides identified public perceptions of Kennedy as "too slick" and "lack[ing] depth," they repeatedly recommended that the campaign develop a "Kennedy-identified program" that would appeal to two distinct groups of voters: ideologically oriented party activists and more moderate centrist voters.

In response to criticism from the left wing of the Democratic party, an aide urged Kennedy in 1959 to address in "clear and unmistakable terms" the "prevailing doubt . . . that Kennedy is a bonafide liberal." The campaign, he proposed, needed to develop an image of Kennedy that would "enable people to be for something [and someone] that excites their imagination."[22] Similar anxieties about Kennedy's image emerged immediately following the presidential conventions, with aides agreeing that the campaign had reached its "low point." In response, Schlesinger suggested that "the present lethargy of the campaign" be reversed by "giv[ing] the enthusiasts something to believe in": "Once the issue-minded Democrats catch fire, then the campaign will gather steam."[23] Kennedy's issue positions, then, were understood by campaign officials as a mechanism, at least in part, to charge the party faithful with "crusading urgency."

But campaign officials sought to identify a "package" of issues that both appealed to party activists *and* responded to the concerns of the centrist voter. They expected that party activists would support (rather than resist) popular positions. Thus campaign officials decided to fashion a "move-ahead" image for Kennedy that would demonstrate a large difference between the Republican and Democratic candidates. The objective, in particular, was to identify Kennedy with new and dynamic approaches to the country's problems, while tying Nixon to eight years of Republican "do-nothingism." Campaign officials calculated that emphasizing Kennedy's proposals for addressing several highly salient issues, which were accessible to the average voter, would serve as a "campaign tool" for creating an appealing image. Thus, in the final phase of the campaign, Harris pressed Kennedy to "go on the offensive" to press his "basic theme on all the issues: to get America on the move at home and abroad."[24]

While an overarching, "move-ahead" image for

Kennedy could reasonably be expected to appeal to reform-oriented party activists, it was the perceptions of the centrist (or median) voter that primarily concerned the campaign. Two criteria, then, were especially critical in the campaign's decisions: (1) identifying issues that were highly salient and accessible to the public and (2) using the activation of voters' attitude structures regarding these issues to demonstrate large differences between the candidates. The first criteria involved *responding* to public opinion, the second, *directing* or manipulating voters' evaluations of Kennedy's personal character.

CAMPAIGN RESPONSIVENESS TO PUBLIC OPINION

Our quantitative analysis confirms that Kennedy's positions responded to public opinion: the issues that were raised in Louis Harris's polls were persistently mentioned by Kennedy in his subsequent public statements. What is significant is not just that Kennedy cited issues identified by the public but that the frequency and strength of his stance was congruent or consistent with the public's preferred *direction* for policy.

The following types of issues, which were salient to respondents in Harris's surveys, were also publicly advocated by Kennedy in the debates and in his public statements (as compiled by the Senate): increasing Social Security, passing Medicare legislation, reforming education, fighting unemployment, combating the high cost of living, and such foreign policy issues as bolstering America's military spending and general prestige.

The relationship between the policy goals that Harris's respondents consistently identified and the candidate's (subsequent and congruent) positions was most clearly evident in Kennedy's public statements. The last row of Table 1 reports overall results. When we compared Harris's weekly polling results (allowing for the appropriate one- or two-week lag) with Kennedy's subsequent positions, we found that Kennedy took congruent positions on 56% of the issues cited in the polls, compared with 27% of the issues not cited in the polls—a 29% difference.

The regression results in Table 2 confirm these findings. The last line of the table indicates that the average correlation (r) between the percentage of public support for a policy goal each week and the position Kennedy staked out was .38. The slope coefficient (b) of .42 for the regression of candidate position on public opinion indicates that for each 2% or so of the public citing a particular policy during a given week, there was one additional congruent policy statement (coded 1 in our content analysis) during the subsequent weekly interval.

These overall findings, however, mask critical variations over the course of the campaign. The strongest relationships are especially evident during the first week in October and the closing weeks of the contest. For the first week of October, the percent difference (37%) jumped more than 20 points from the previous week's level (16%) and was 8 points higher than the

TABLE 1

Relationship between John Kennedy's Poll Results and Subsequent Public Statements during the 1960 Election (%)

WEEK OF CAMPAIGN	ISSUES ON WHICH KENNEDY TOOK POLICY STANCES DURING GIVEN WEEK: NOT CITED IN POLLS %(N)	CITED IN POLLS %(N)	DIFFERENCE
Opening[a]			
September			
Week 3	26 (31)	54 (24)	28*
Week 4	13 (31)	29 (24)	16
October			
Week 5	41 (37)	78 (18)	37*
Home stretch[b]			
Week 7	22 (32)	52 (23)	30*
Week 9	35 (31)	62 (24)	27*
November			
Week 10	27 (33)	64 (22)	36*
Average	27	56	29*

Note: On issues cited in Kennedy's polls, we report the percentages of policy stances consistent with public opinion. Numbers of issues are in parentheses. No analysis is shown for weeks 1, 2, 6, and 8 because there were no polls in the appropriate preceding period.

[a] Two-week lag between polls and statements.

[b] One-week lag between polls and statements.

*$p \leq .05$.

TABLE 2

Effect of Kennedy's Poll Results on the Content of His Public Statements during the 1960 Election

WEEK OF CAMPAIGN	BIVARIATE REGRESSION (b)	CORRELATION COEFFICIENTS (r)
Opening[a]		
September		
Week 3	.37*	(.33*)
Week 4	.08	(.15)
October		
Week 5	.40*	(.30*)
Home stretch[b]		
Week 7	.55*	(.54*)
Week 9	.78*	(.54*)
November		
Week 10	.32*	(.41*)
Average	.42*	(.38*)

Note: The independent variable is the average percentage of respondents in Kennedy's polls ranking an issue and policy direction as important. The dependent variable is the sum of the content scores for Kennedy's statements about the issues, where each statement was coded on a five-point scale (−2 to +2) depending on the degree of support for the policy. N = 55. No analysis is shown for weeks 1, 2, 6, and 8 because there were no polls in the appropriate preceding period.

[a] Two-week lag between polls and statements.

[b] One-week lag between polls and statements.

*$p \leq .05$.

average (29%). Fully 78% of the issue positions cited in Harris's surveys two weeks earlier were echoed by Kennedy during the campaign's fifth week, compared to 41% of the issues that did not appear in the polling reports. Similarly, the regression coefficient rose to .40 (r = .30) from previous week's figure of .08 (r = .15).

The most striking results occurred during the closing weeks of the campaign. Table 1 suggests that week 10 produced a 36-point percentage difference, 9 points higher than the previous week. Table 2 confirms this overall picture of heightened responsiveness during the campaign's home stretch. In particular, the b coefficient for the campaign's ninth week was .78, nearly twice the size of the average figure (.42). It was also larger than the figure (.55) for the previous week with available data—week 7. For each 5% of the public citing a particular policy during the campaign's eighth week, there were two additional directly congruent policy statements (coded 2 in our content analysis) during the following week. Kennedy's elevated responsiveness is illustrated by his handling of unemployment during campaign's home stretch: a threefold increase in Harris's polling results during the second week in October (from 4% to 13%) was associated with a fourfold increase in Kennedy's statements the following week (from a policy statement score of 6 to a score of 26).

Tables 1 and 2 present an interesting contrast for week 9: the percentage difference is average, whereas the b coefficient is the largest of any week. Although Kennedy was not more apt than on average to raise an issue that the public cited, the results that Harris reported to Kennedy apparently had an exceptional impact on the number and strength of his statements: Kennedy was acutely responsive to the issues that were highly salient and accessible.

Kennedy's heightened responsiveness during the campaign's final weeks is consistent with the strategic importance of this period for priming voters. The attitudes structures that have been most recently activated from the electorate's short-term memory will be especially decisive in voters' choices. Indeed, it was during the campaign's final push that Harris pressed Kennedy to "go on the offensive" by advocating salient issues.[25]

The statistical results for Kennedy's positions during the two debates for which there were preceding opinion surveys are also revealing. The last row of Table 3 shows that in the debates Kennedy was, on average, 41% more likely to promote issues that were cited in previous polls than to raise issues that lacked public attention and support: he made congruent statements on 49% of the positions cited in Harris's reports compared to 8% for issues not cited in the polls. The last row of Table 4 presents the average regression results: the b coefficient is .18, and the correlation coefficient is .47. Overall, then, an issue's citation in Harris's reports made it quite likely that Kennedy would raise the issue, but the impact of reported opinion on the strength and frequency of Kennedy's statements, as estimated by the regression b coefficient, was not strong.

TABLE 3

Relationship between John Kennedy's Statements during the Debates and His Most Recent Poll Results (%)

DEBATE (SUBJECTS)	NOT CITED IN POLLS %(N)	CITED IN POLLS %(N)	DIFFERENCE
Opening[a]			
26 September (domestic policy)	11 (37)	56 (18)	45*
Home stretch[b]			
13 October (subject open)	6 (32)	43 (23)	37*
Average	8	49	41*

Note: On issues cited in Kennedy's polls, we report the percentages of policy stances consistent with public opinion. Numbers of issues are in parentheses. No analysis is shown for October 7 and October 21 because there were no polls in the appropriate preceding period.

[a]Two-week lag between polls and statements.

[b]One-week lag between polls and statements.

$^{*}p \leq .05$.

Kennedy's was most responsive during the first debate, which occurred on 26 September. Table 4 indicates that the b coefficient for the first debate was more than twice that for the other debate (.26 vs. .11). Moreover, the 45% difference in Table 3 is the highest figure to emerge in our analysis: Kennedy made congruent statements during the debates on 56% of the issues cited in Harris's reports, compared to 11% on issues not cited in the polls.

The strong findings for the first debate may reflect a general *trend* toward responsiveness during this period. The results for Kennedy's public statements,

TABLE 4

Effect of Kennedy's Poll Results on the Content of His Statements during the Debates

DEBATE (SUBJECT)	BIVARIATE REGRESSION (b)	CORRELATION COEFFICIENTS (r)
Opening[a]		
26 September (domestic policy)	.26*	(.48*)
Home stretch[b]		
13 October (subject open)	.11*	(.46*)
Average	.18*	(.47*)

Note: The independent variable is the average percentage of respondents in Kennedy's polls ranking an issue and policy direction as important. The dependent variable is the sum of the content scores for Kennedy's statements about the issues, where each statement was coded on a five-point scale (−2 to +2) depending on the degree of support for the policy. N = 55. No analysis is shown for October 7 and October 21 because there were no polls in the appropriate preceding period.

[a]Two-week lag between polls and statements.

[b]One-week lag between polls and statements.

$^{*}p \leq .05$.

which are presented in Tables 1 and 2, also found that public opinion became more influential during and after the debate (compare weeks 4 and 5 in Tables 1 and 2). However, the statistical results for the debates should not be overstated: analysis of the first debate is limited to a relatively few statements during a short period—one evening. In addition, reported opinion had only moderate impact at best on the frequency or strength of Kennedy's statements. The media's role may explain why reported opinion had a more limited impact on the debates than on Kennedy's public statements: because journalists asked the questions, they (not Kennedy) controlled the agenda and the set of issues that the candidates addressed. Kennedy's reduced responsiveness during the debates may reflect the media's intervention.

The general pattern of our findings suggests a striking and consistent relationship between Harris's surveys identifying a policy issue as important and Kennedy taking a subsequent and corresponding position on it in his public statements and in the debates.

Kennedy, however, did take positions on such issues as civil rights and foreign aid that the public either failed to fully support or openly opposed. On the issue of civil rights, Harris reported responses to the following three related issues: antiintegration, prointegration, and states' rights. These three issues were identified in nearly each of the 26 surveys conducted.

Public opinion toward civil rights clearly divided on regional grounds. Respondents in critical northeastern states like New York and Pennsylvania consistently ranked prointegration among their top 10 concerns; segregationist positions (opposing integration and favoring states' rights) were not identified as major concerns. States in the Democrats' traditional stronghold of the South held nearly opposite preferences: states like South and North Carolina identified antiintegration as their top policy concern.

Kennedy cautiously reacted to these regional divisions in public opinion (and in the Democratic party). On the one hand, he spoke in favor of expanded civil rights, a position that went against the antiintegration attitudes in the South. On the other hand, he had sufficient information about southern opinion to appreciate the benefit of crafting an ambiguous position and minimizing the issue's exposure during the campaign. Thus, when the issue was raised, Kennedy emphasized his narrow, legalistic concerns and endorsed civil rights based on constitutional (rather than moral) grounds. Kennedy did his best, though, to downplay the issue and its salience: over the course of three months of continuous daily statements, he only made statements on the issue a couple dozen times. One of the only wrinkles in the campaign's avoidance of the potent issue of race was caused by Kennedy's call to Coretta Scott King on the eve of the election. Robert Kennedy's initial reaction was that contacting King had "lost" or "screwed the whole election up." He feared that the call had attracted attention to the issue of civil rights as one of the standards on which the candidates would be judged (according to Sargent Shriver, quoted in Jackson and Riddlesperger 1991, 5). In other words, the fear was that the candidate's action had activated voters' memories about a set of issues that could only hurt their evaluation of John Kennedy: his understated position would satisfy neither southerners nor integrationists.

Too much foreign aid was another position with which Kennedy did not publicly agree or follow public opinion: this position was persistently cited by a relatively high proportion of Harris's survey respondents, but Kennedy repeatedly staked out an opposing position in the debates and in his public statements. As Harris emphasized in our interview with him, foreign aid was an area about which Kennedy deliberately "took a stand in spite of public opinion" in order to assure other elites of his worthiness to be president. Instead of deferring to public opinion, he devoted an exceptional amount of time (especially during the October debates) attempting to educate the American public about a foreign policy that advanced the country's national interests.[26]

Issues like foreign aid and civil rights, however, are clearly exceptions. In general, Kennedy was much more likely to take stances on issues that Harris identified as salient and popular than on policies that lacked this kind of support.

Archival records of Kennedy's primary and general election campaigns corroborate the statistical evidence that the campaign consistently selected issue areas and particular policy directions that were salient and accessible to Americans. The issue of passing Medicare legislation, which would help finance the elderly's health care, illustrates this sensitivity to public opinion (Jacobs 1993, 1992a).

The tireless reformer, Wilbur Cohen, deliberately capitalized on the campaign's concern with public opinion when he pressed Kennedy to promote Medicare: Cohen assembled recent polls in order to be able to tell "Kennedy and his people: this proves . . . we've got public opinion."[27] Moreover, Harris also stressed that he found strong polling results favoring Kennedy's medical care position: he repeatedly identified it as among "the most dominant overall-all problem[s] of concern to voters and the sharpest cutting edge[s]" between the candidates. In his polling reports and verbal advise to Kennedy, he concluded that "there is no situation, no local or national speech, in which the medical care issue should be ignored."[28]

Kennedy's recognition of this "public interest and apparent support" prompted him to select Medicare for particular emphasis. As one campaign official recalled, Kennedy believed that it was, "of all the issues on which he campaigned, . . . the one that constantly provoked the most interest." Kennedy's perception of Medicare's salience (one shared by other campaign officials) prompted him to select it from a "roster" of issues that he already supported for particularly frequent and strong statements.[29] Indeed, a careful look at Harris's findings and Kennedy's statements suggests a close relationship. For instance, a more than twofold increase in public support for Medicare during mid-September (from 9% to 22%) coincided—two weeks later—with both a sevenfold increase in Kennedy's daily attention to the issue (from a policy statement score of 2 to a score of 14) and a decision to select Medicare for exceptionally frequent, strong, and extensive statements during the first debate. In short, Kennedy's polling information prompted him to make tactical adjustments, modifying the extensiveness and forcefulness of his positions.

USING SALIENT POSITIONS TO BUILD CANDIDATE IMAGE

Evidence of Kennedy's responsiveness—that he consistently took positions on specific policy areas and options that were salient and accessible to Americans—suggests that information about public opinion from his private polls was strategically attractive to the candidate because it diminished his uncertainty regarding voter preferences (Geer 1991). The responsiveness to the public's concerns about policy issues contradicts the conventional assumption in campaign analysis that rational candidates avoid position taking. What remains unanswered, however, is how Kennedy's information regarding the public's policy concerns was understood and used in strategic terms by the campaign. Conceivably, Kennedy's responsiveness could have been motivated by Downsian-type calculations: he may have positioned his issue stands in close proximity to the public's policy concerns, expecting voters to make a prospective decision that his election would secure desired benefits. Alternatively, the campaign's calculations may have implicitly reflected a strategy of priming. That is, the strategic objective of adopting positions on popular issues may be to emphasize standards for evaluating a candidate's personal characteristics, which encourages voters to perceive large—and advantageous—differences in the candidates' images.

The Kennedy campaign viewed position taking on salient, accessible issues as the means by which to project the candidate's "move-ahead" image.[30] It was "through the issues" and by "hammer[ing] home" his positions on medical care, education, and other policies that the campaign attempted starkly to contrast Kennedy's image (as "caring" and willing to offer new approaches) with Nixon's identification with the Republican's "drift and inaction."[31] "The most fundamental job," Harris advised, was to "increase Kennedy's identification . . . with the problems important to voters" and thereby to demonstrate that Kennedy, unlike Nixon, "really cares" about addressing the problems facing the country and individual voters.[32] Thus, although medical care was expected to have special meaning for liberals and the aged, the medical care issue was also seen as a way to shape Kennedy's image to "many voters other than the old people themselves."[33]

The Kennedy campaign's decision to use popular policy issues as part of a priming strategy raised two strategic concerns. First, the campaign struggled to identify Kennedy visibly with these popular issues. It used a range of strategies (including Senate activities[34] and a "TV spot campaign")[35] to cement Kennedy's identification with popular issues. For example, in preparation for the debates, Kennedy's advisers analyzed the most effective issues, with Harris identifying "leading domestic issues as they emerge from our surveys" (medical care and education topped his list). As part of this effort to make sure that Kennedy impressed voters with his concern about salient issues, the campaign monitored whether and with what frequency Kennedy discussed these issues during the debates.[36]

Second, as the general election campaign developed, Kennedy's advisers endeavored to protect Kennedy's identification with popular issues from Nixon's efforts to (as Clark Clifford put it) "fuzz up" the differences.[37] For example, Nixon's concern with the Democrats' use of the popular Medicare issue led him to reverse the Eisenhower administration's previous opposition to new legislation and to "resort to me-tooism." Nixon stressed that despite the differences in the approaches favored by the candidates, he shared Kennedy's aim of helping the aged finance their health care (David 1985; Jacobs 1993, chap. 5; Page 1978, 73–74).

In other words, Nixon was attempting to counter Kennedy's strategy of priming salient issues in order to portray large differences in each candidate's character. The Republicans intended to neutralize the impression that Medicare illustrated Nixon's listless character as opposed to Kennedy's forward-looking approach. Nixon's actions are consistent with theoretical models about rational candidate behavior: he used information following Kennedy's announcement to better estimate the true position of voters and decided to modify his position accordingly.[38]

Alarmed at Nixon's attempt to create the "illusion" of similarity between the candidates' positions, Kennedy's aides repeatedly urged their candidate to emphasize the differences on the issues.[39] If popular issues were to be an effective "tool" in shaping public evaluations, the campaign felt compelled to protect and reinforce Kennedy's identification with those issues. Thus, as election day approached, Harris frequently warned Kennedy to resist Nixon's "fuzz-up" effort and his own campaign's tendency to "move over to new issues" by "dramatizing" and "pressing home" the candidates' differences.[40]

DISCUSSION

We modified social psychologists' study of priming by shifting its focus from individuals to candidate behavior and by accounting for the tactical considerations and intentional strategies of modern campaigns. Partly because position taking risks alienating centrist voters, candidate priming is expected to respond to salient and accessible issues: campaigns emphasize these issues in order to influence voters' evaluations of their candidate's personal image and to demonstrate large appealing differences from their competitors.

Our analysis of Kennedy's strategy in the 1960 race confirms two critical aspects of a candidate priming strategy. First, we found that Kennedy responded to private polling information that identified specific policy areas and policy directions that were highly salient and accessible to Americans. Statistical evidence concerning the impact of reported opinion was reinforced by interviews and archival evidence. In its efforts to fashion a winnable election strategy, the Kennedy campaign used its polling results to select and emphasize a limited number of policy issues from a large pool of possible choices.

Some of Kennedy's positions (e.g., his support for health insurance for the aged) were enduring "party cleavage" issues, over which Democrats had been battling Republicans for decades. It could naturally be anticipated that the relationship between Kennedy's positions and Harris's polling results was spurious: it is possible that the candidate's positions were driven not by polls but by longstanding divisions between the political parties and their supporters. In other words, party activists (rather than the electorate) may have been the driving force.

Although partisan cleavages obviously played a role in the 1960 elections, the archival and statistical evidence strongly suggests a causal connection between Kennedy's polls and the positions he took. While partisan division might explain why various issues would be included in the pool of conceivable election issues, the evidence indicates that Kennedy's campaign was quite selective regarding which policies it chose from this pool of party cleavage issues.[41] Moreover, some of Kennedy's positions (such as his foreign policy stance on the new issue of rebuilding American prestige) were not issues that had persistently divided the parties.

Kennedy's concern with responsiveness to public opinion was also reflected in his decision to commit scarce financial and organizational resources to polling and to incorporating these assessments of public opinion into the campaign's decisions. Indeed, the campaign's organizational and strategic interest in assembling private poll data was a watershed event in the evolution of modern campaigning. It is well known that past politicians conducted polls; but the number, quality, and strategic use of Harris's surveys represent significant innovations. Indeed, Kennedy and his aides believed that their recognition of the "importance and value [of polling results] on the American scene" and on the designing of campaign strategy were historic firsts.[42] The campaign's pioneering use of polling to improve its information about voters' concerns had an effect beyond the 1960 race: the campaign prompted Kennedy to inaugurate private polling as an enduring, institutional commitment of the White House (Jacobs 1992b; 1993).[43]

Our second finding is that Kennedy's responsiveness coexisted with his attempt to manipulate voters' evaluations of his *personal character*. Kennedy's responsiveness to the issues that the public ranked as important was driven neither by an exclusively image-based strategy nor by a straightforward Downsian calculation that the proximity of his issue stances to voter's preferences would win over the electorate. (Of course, Kennedy's campaign did not use or consciously test these competing theories.) Instead, the campaign viewed Kennedy's advocacy of a few popular policy issues as a political resource for highlighting the candidate's bold, competent proposals for "moving ahead." By emphasizing a few salient, accessible issues in voters' memories, the campaign intended to shape the electorate's standards for evaluating and judging competing office seekers. The objective was to use priming to construct an image that would be appealing and noticeably divergent from Nixon's.

Although comparable research on other presidential elections has not been conducted, Kennedy's use of salient issues to shape candidate image may provide insights into subsequent campaigns—even those in which candidate behavior apparently followed Downsian expectations. For instance, the 1964 and 1972 presidential contests appear to be classic illustrations of candidates offering divergent policy stands. Goldwater and McGovern presumably lost because their opponent's centrist positions promised greater benefits to the median voter than did their own extreme proposals. Nevertheless, further analysis of each candidate's issue stands may well find the voters' decisions in 1964 and 1972 resulted from the victorious candidate's use of a priming strategy. By focusing the electorate's attention on Goldwater's and McGovern's initial, isolated, and often retracted "extreme" issue stances, the eventual victor in each race was able to mobilize (with the media's assistance) widely shared beliefs that cast their opponents in a bad light (Kelley 1983; Page 1978; Popkin 1991).[44]

CONCLUSION

Our findings have two broad implications for the future study of elections. A fuller assessment of these implications than is possible here, however, requires that our empirical analysis of one campaign be supplemented with research on additional campaigns and on voting behavior. First, the process of priming may provide an analytic basis for recoupling research in the two subfields of voters' motivation and campaign strategy. For decades, the two interrelated components of electoral analysis—research on voters and on candidates—were held together by a shared emphasis on the influence of candidates' personal image and on the limited role played by policy issues. The limits on Downsian expectations regarding issue-oriented campaigns were compounded by a number of stable obstacles to issue-guided vote choice: voters' incomplete or inaccurate information, their tendency to make retrospective judgments of government performance, and their party loyalties (Campbell et al. 1960; Fiorina 1981; Keith et al. 1992; Smith 1989).

The conventional wisdom, however, that rational campaigns adopt nonissue strategies that emphasize favorable personal images has been undermined by the recent revisionist interpretation of the American electorate (e.g., Aldrich, Sullivan, Borgida 1989; Popkin 1991; on policy preferences more generally, see Page and Shapiro 1992). The finding that voters are influenced (perhaps decisively) by policy issues has effectively decoupled current analysis of voters from the study of campaigns; voting studies consider issues quite consequential for voters, whereas the analysis of campaigns treats candidate strategy and behavior as non-issue oriented.

If analysis of campaigns is to be reintegrated with the study of voting, new approaches that recognize policy issues as an important basis for vote choice must be developed for investigating candidate behavior. Research on priming offers one approach for conceptually linking the analyses of campaign behavior and vote choice. Incorporating candidate behavior in the study of voters would mean treating the influences on vote choices as *endogenous* to the campaign (i.e., the impact of deliberate strategies that candidates pursue in order to win over voters). On the campaign side, priming is useful for refocusing campaign analysis on candidates' treatment of issues. Kennedy's 1960 candidacy illustrates how one campaign combined image building with a candidate's position taking on issues that responded to perceived public opinion.

Second, the Kennedy campaign's collection and use of polling information raises complex normative issues that defy simple categorization. On the one hand, the campaign's sophisticated use of opinion surveys might be considered consistent with a conception of liberal democracy in which political leaders both respond to and direct public opinion. In terms of democratic responsiveness, Kennedy was systematically guided by polling on voters' concerns. Moreover, liberal democracy is also premised on politicians directing or educating the mass public on critical issues of national importance. Kennedy's determined efforts to move public opinion toward embracing his position of backing foreign aid suggest that on this issue, he did alter the balance between leading and following public opinion. In addition, Kennedy's success in using a strategy of this sort may confirm the role of competitive elections in leadership selection. The Democrat's victory could be interpreted as foreshadowing his effectiveness as a national leader.

On the other hand, Kennedy's campaign strategy does have a disturbing implication for integrating elections into the governing process of America's liberal democracy.[45] In an ideal situation, competing candidates would respond to public opinion (and to pressure from party activists) by offering important and constructive policy positions. Voters' choices in the electoral process could then identify clear directions for subsequent government decisions. In effect, this style of competitive elections would fulfill the long-denied objectives of responsible party advocates: elections would not only serve as formal processes for determining succession but would be linked to choices over basic policy direction (Lowi 1967, 1985; Schattschneider 1948).

Kennedy's 1960 campaign devoted enormous resources to identifying the public's policy goals, but this information was used to influence evaluations of Kennedy's personal qualities. For responsible party advocates, then, the recoupling of voters and campaign strategy in this way is not promising: candidates incorporate issues into their strategy but they do not bank on substantive (and popular) policy positions to win their contests. Kennedy's selective use of issues meant that no clear meaning could be attached to his electoral victory; there was no mandate or popular confirmation of a policy platform to pursue in governing once he took office.

The emergence of this form of priming is consistent with a political regime that has often failed over much of the last two centuries to incorporate policy decisions into the electoral process (see Ginsberg 1972, 1976; Lowi 1967, 1985). Candidates' efforts to prime voters in order to manipulate their evaluations is but one more reason that Americans' choices of officeholders rarely settle policy issues.

Notes

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1. Analysis of strategic position taking differs from Downs's own work in several critical respects. In contrast to what is often considered Downsian analysis, Downs himself emphasized the role of nonissue considerations (1957, esp. pt. 2) and, moreover, focused on the behavior of political parties, rather than individual candidates.

2. It is not necessary to pinpoint the process by which attitudes become accessible—whether retrieved via chronic or temporary accessibility. It is reasonable to assume that even if attitudes are chronically accessible, the retrieval of such attitudes is enhanced by campaign priming (Aldrich, Sullivan, and Borgida 1989).

3. While we focus on candidate priming strategies, we recognize that voters face multiple sources of priming.

4. In fact, spatial models have taken this into account

mathematically by including a set of weights, which accounts for the relative importance of issues (e.g., the "a-matrix"; see Davis, Hinich, and Ordeshook 1970). One complication, however, is that different issues may be important to different individuals: the weights theoretically can vary across individuals. In addition—and most important—the weights are variables. They are not given.

5. The simulation project's input into the Kennedy campaign was largely limited to the period before the general election campaign. But, Pool, Abelson, and Popkin concede that their studies were not viewed as useful by Kennedy's aides, who relied on Louis Harris's studies (1964, 18).

6. We generally focus on responses to the following type of open-ended question: "Now, what do you feel are the two or three biggest problems facing people like yourself that you feel the *national* government should do something about in the next four years? Any others?" As we shall discuss, Harris reported not only the ranking of a policy's importance but also the policy direction that respondents preferred.

7. Harris to JFK, 23 July 1959, box 24, JFK Theodore Sorensen Papers, Kennedy Library; Harris to JFK, memo, 5 July 1960, box 39, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

8. JFK to Harris, memo, 5 July 1960, box 39, Political, Pre-Administration, RFK Papers, Kennedy Library; interview with Harris by authors, 17 June 1991.

9. Interview with Harris by authors, 17 June 1991.

10. Ibid.

11. Ibid.

12. Archival records and our interview with Harris suggest that Kennedy treated the state public opinion data as representative of national opinion. It is on this basis that we compare the responses of Harris's state samples with Kennedy's policy statements.

13. The debates occurred on 26 September, and 7, 13, and 21 October, with the first focusing on domestic policy and the last on foreign policy (the subjects of the other two debates were open).

14. Interview with Harris by authors, 17 June 1991; interview with Helen Lempart by N. Aldrich, March 1966, Kennedy Library; "Memorandum on the First Kennedy-Nixon Debate on Domestic Issues 22 September 1960, box 36" Political, Pre-Administration, JFK/RFK Papers; "Index to topics discussed and list of topics not discussed by Senator Kennedy during the first four debates."

15. A subcommittee of the Senate Commerce Committee assembled a four-volume set of the public positions taken by Kennedy and Nixon as part of its investigation into the dissemination of political information during the 1960 general election campaign. We coded what were clearly Kennedy's main substantive policy statements in part 1 (speeches, remarks, press conferences, and statements) and part 3 (radio and television appearances) (United States, Congress, Senate 1961). Further information is available from the authors.

16. We prepared detailed written instructions and held frequent discussions with two graduate research assistants concerning coding rules and treatment of problematic cases. We independently coded random samples of Kennedy's statements and the content of the debates to verify that the instructions were followed. Coding discrepancies of more than one unit of our five-point scale occurred on approximately 5% of the assistants' and authors' coding. Finally, the coders were unaware of the polling results and therefore were unaffected by prior knowledge concerning our opinion measure.

17. For instance, if Kennedy took 3 positions within one week on the issue of civil rights and these statements were coded +2, −2, and +1, the weekly measure for the content would be +1.

18. Any issues omitted from the debates, the Senate material on daily statements, and Harris's polls (and thus excluded from our data) would have led to underestimates of the correlation between the poll reports and Kennedy's visible positions. Moreover, on opposing Communist China and issues related to Catholicism, Kennedy actually had additional opinion data that we did not incorporate in our statistical analysis. On the issue of Catholicism, Harris provided substantial evidence separate from his "important problem" series; on the issue of China, Harris reported general opposition to communism, which may have influenced Kennedy's more specific policy toward China. (We coded the candidate's position toward China as a separate issue.)

19. Harris did telephone Kennedy with preliminary results. Although horse-race reports on Kennedy's standing against Nixon could easily be conveyed verbally, the more cumbersome data on policy issues were reported last and took the longest time to incorporate into campaign strategy. Interview with Harris by authors, 17 June 1991.

20. The use of one-week lags during September produced statistically insignificant results, which is consistent with the expectation that Kennedy's strategists did not receive and incorporate Harris's results until some two weeks after the survey's completion.

21. J. Miller, "Some Modest Realignments in the Kennedy Image," Memo, 20 October 1959, box 39, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Wilbur Cohen by Jacobs, 1 April 1987; interview with Cohen by David McComb, 8 December 1968.

22. Miller, "Some Modest Realignments."

23. Schlesinger to JFK, 26 and 30 August 1960, Special Correspondence, box 32, JFK/Presidential Office Files, Kennedy Library; "Memorandum on the Last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

24. Louis Harris and Associates, "An Analysis of the Third Kennedy–Nixon Debate," 19 October 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Harris by authors, 17 June 1991.

25. Louis Harris and Associates, "An Analysis of the Third Kennedy–Nixon Debate," 19 October 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

26. Interview with Harris by authors, 17 June 1991.

27. Interview with Wilbur Cohen by Jacobs, 1 April 1987.

28. Interview with Harris by authors, 17 June 1991; Louis Harris and Associates, "A Study of Presidential Election in California" and "A Report of the Presidential Race in Maryland," September 1960, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Louis Harris and Associates, "A Report on the Presidential Election in Maine," box 44, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

29. Interview with Harris by authors, 17 June 1991; interview with Ivan Nestingen, 1966, Social History Research Office, Columbia University. Leaders of the Kennedy campaign in the states during both the primaries and the general election recalled support for the medical care issue; these leaders included Nestingen in Wisconsin; Anthony Akers in New York; and Charles Love, Jr. in West Virginia. In addition, close advisers also reported the same favorable reaction. Interview with Akers by William Moss, 17 July 1971, Kennedy Library; interview with Love by William Young, 14 July 1964, Kennedy Library; Abraham Ribicoff to RFK, 15 October 1960, box 25, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; "Memorandum on the Last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

30. "Memorandum on the Last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library. This theme is also prevalent in Harris's polling reports.

31. Louis Harris and Associates, "A study of Presidential Election in Ohio, Wave I," 6 September 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; "Memorandum on the First Kennedy–Nixon Debate on Domestic Issues," 22 September 1960, box 36, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Louis Harris and Associates, "A Study of Presidential Election in California, Wave III," 31 October 1960, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

32. "A Report of the Presidential Race in Maryland," Louis

Harris and Associates, Inc. September 1960. This is a constant theme in Harris's reports.

33. Louis Harris and Associates, "A Study of Presidential Election in Florida," 13 September 1960, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Louis Harris and Associates, "A Study of Presidential Election in Maryland, Wave I," 23 September 1960, box 44, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; idem, "A Survey of the Presidential Election in South Dakota," 19 September 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; "Simulatics, Report #2," Memo, 25 August 1960, box 48, Political, Pre-Administration, JFK/RFK Papers, Kennedy Libraries.

34. For instance, in preparation for his presidential candidacy, Kennedy presented to the Senate, in August 1958, a 10-point program to establish a "Bill of Rights for the Elderly," in which health insurance was one of the top priorities. In addition to formal statements, Kennedy cosponsored a Medicare bill in the Senate during the campaign in spring 1960; by the fall, he cosponsored an important amendment that would establish a health insurance program for the elderly.

35. Steve Smith to Campaign Coordinators, memo, 19 October 1960, box 46, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

36. "Index to topics discussed and list of topics not discussed by Senator Kennedy during the first four debates" and "Memorandum on the First Kennedy–Nixon Debate on Domestic Issues," 22 September 1960, box 46, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Helen Lempart by N. Aldrich, March 1966, Kennedy Library.

37. Clark Clifford to JFK, memo, 27 September 1960, box 36, Political, Pre-Administration JFK/RFK Papers, Kennedy Library.

38. The interrelationship between Kennedy's and Nixon's behavior underscores the fact that campaigns involve strategic interactions, rather than a one-player game. Neither space nor comparable data enable us to explore here this important component of campaign strategy.

39. Interview with Clinton Anderson by John Stewart, 14 April 1967, Kennedy Library. "Memorandum on the last Nine Days of Campaigning," confidential memo, box 43, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; George Belknap, "Most Americans Are Basically 'New Dealish'," June–August 1960, Box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; Abraham Ribicoff to RFK, 15 October 1960, box 25, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

40. Louis Harris and Associates, "A Study of Presidential Election in Ohio, Wave II," 19 October 1960, box 45, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library.

41. For instance, while health insurance is an obvious issue on which the political parties disagreed, Kennedy's proposal differed in important respects from the last major Democratic plan, Truman's universal health insurance proposal.

42. JFK to George Gallup, 24 July 1959, box 24, JFK/Theodore Sorensen Papers, Kennedy Library; Harris to JFK, 23 July 1959, box 24, JFK/Theodore Sorensen Papers, Kennedy Library; Harris to JFK, 5 July 1960, box 39, Political, Pre-Administration, JFK/RFK Papers, Kennedy Library; interview with Harris by authors, 17 June 1991.

43. Johnson significantly expanded the White House's polling operation (Jacobs and Shapiro 1992).

44. Future research may find that the campaign responsiveness to public opinion can lead to strategies that are quite different from that which Kennedy pursued. For instance, reported opinion led the 1988 Bush campaign to avoid economic issues, which were the top public concerns, and instead to focus attention on the previously quiet issues of crime and patriotism (Moore 1992).

45. Lying or misleading the public would be another serious threat to liberal democracy; evidence of this type of manipulation occurred in the case of the alleged U.S.–Soviet "missile gap" (see Key 1961; Page and Shapiro 1992, chaps. 6, 9–10).

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ENHANCING DEMOCRACY THROUGH LEGISLATIVE REDISTRICTING

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We demonstrate the surprising benefits of legislative redistricting (including partisan gerrymandering) for American representative democracy. In so doing, our analysis resolves two long-standing controversies in American politics. First, whereas some scholars believe that redistricting reduces electoral responsiveness by protecting incumbents, others, that the relationship is spurious, we demonstrate that both sides are wrong: redistricting increases responsiveness. Second, while some researchers believe that gerrymandering dramatically increases partisan bias and others deny this effect, we show both sides are in a sense correct. Gerrymandering biases electoral systems in favor of the party that controls the redistricting as compared to what would have happened if the other party controlled it, but any type of redistricting reduces partisan bias as compared to an electoral system without redistricting. Incorrect conclusions in both literatures resulted from misjudging the enormous uncertainties present during redistricting periods, making simplified assumptions about the redistricters' goals, and using inferior statistical methods.

In 1982, the Michigan Supreme Court imposed a redistricting plan on their state that was generally believed to favor the Republicans. The Democrats' alternative measure had to pass the legislature with a two-thirds vote, which was difficult even though they had a majority in both houses. Democratic leaders tried to sneak through the legislation by gutting the contents, but not the title, of an irrelevant bill at the last minute and inserting redistricting legislation. The Republicans discovered this ploy, making the situation extremely tense. In the heat of the long debate during this midnight session, a Democratic senator collapsed. Paramedics were called in, but he refused to leave the Senate floor before the vote. In a classic case of political hardball, a Republican senator used parliamentary procedure to delay the vote by insisting that the legal description of all 148 legislative districts and their boundaries be read into the record. Despite his failing health, the Democratic senator stayed through the entire reading, and his party won the vote.[1]

During the Illinois redistricting process,

> Republican state Senator Mark Rhoads believed he had the Democratic votes needed to pass a GOP map in the Senate. In a rare Sunday legislative session Rhoads became outraged over the parliamentary tactics employed by [Senator] Rock to delay a vote on reapportionment. Unable to control his anger, Rhoads attempted to charge the podium and get at Rock. However, before he reached the burly Senate president, Democratic downstate Senator Sam Vadalabene sucker-punched Rhoads with a right to the jaw. According to eyewitness Al Manning of the *State Journal Register,* "for a moment it looked as though both benches were going to empty," but, with the television cameras grinding, the combatants were pulled apart. Later in the day, Rock eventually called the remap bill and with total party unity the Democrats passed out their own bill, thus assuring a reapportionment deadlock. (Green 1982, 32)

These are among the most colorful recent redistricting stories, but they accurately portray the intensity of the partisan conflict in many such processes throughout the United States. From George Washington's first presidential veto to the present day, redistricting issues have been extremely controversial at every level of government. Most redistrictings are contested in state and federal court cases heard so late that there is insufficient time to follow the usual rules of discovery, evidence, or due process. In total, legislative redistricting is one of the most conflictual forms of regular politics in the United States short of violence.

While partisan and bipartisan redistricting plans can protect incumbents, they only protect some of those who survive the redistricting process—and many do not survive. Indeed, most incumbent politicians would give an awful lot to avoid redistricting altogether. After all, they are fighting over the fundamental rules of the game (fights that might well have been concluded at the founding of the republic) and for their own political survival. As a result, redistricting creates enormous levels of uncertainty, an extremely undesirable situation for any sitting politician. Indeed, because the costs of the political fight frequently outweigh the benefits of government service during redistricting, incumbents disproportionately choose to retire at this time.[2]

Some scholars assume that those who draw the district lines are motivated by incumbent protection, whereas others believe the motivation is partisan advantage, but even the briefest discussion with participants in the process indicates that redistricters are concerned with both. Indeed, these are often competing goals: incumbents are often forced to give up votes (hence electoral safety) in order to increase the number of legislative seats their party is likely to capture. The tension between the goals of individual and partisan advantage creates yet additional uncertainty about the outcome of a redistricting. Since political party gain is the most predictable common ground for otherwise competing incumbents, party advantage will often take precedence over individual incumbents' advantage in the ultimate political compromise represented by a redistricting plan.

Moreover, not only do redistricters attempt to maximize the competing goals of incumbency protection and partisan advantage, but incumbency protection is itself composed of competing goals: winning the general election and winning (or avoiding) the primary election. These goals conflict because adding too many of a legislator's political party members to his or her district (hence piling up expected votes in the general election) might leave the incumbent vulnerable to a now larger opposition faction within his or her party primary.[3]

In addition to the high levels of political conflict and uncertainty and the conflicting goals of those who draw the district lines, the entire process includes several severe legal and political constraints. These include the requirements of equal population, contiguity, compactness, minority representation, maintaining communities of interest, not splitting local subdivisions, and especially protecting some incumbents, all within the context of complicated local geography. Other constraints are much less widely recognized but no less important to incumbents, such as the inclusion of the right political contributors, the exclusion of prospective challengers, and the keeping of each favored incumbent's several district offices within the district.[4]

Thus, in our view, the key to understanding the effects of redistricting is to view redistricters as trying to achieve consensus among—or impose a solution on—incumbents who are operating in an extremely uncertain environment and attempting to reconcile at least three competing goals: to maximize their probability of winning or avoiding a party primary, to win a general election (conditional on winning the primary), and to increase their political party's seat advantage. The resulting redistricting plan is usually a compromise, heavily influenced by numerous formal and informal constraints, which generally weights the political party's overall seat advantage most heavily.[5]

We shall evaluate, and then resolve, two important scholarly disagreements about the effects of legislative redistricting on two features of American democratic electoral systems: *electoral responsiveness* and *partisan bias.* Both sides in each debate are inconsistent with part of the substance of redistricting as just portrayed. The results of our analyses define and establish new positions. They do not fully support either side in what were previously portrayed as either/or debates but are consistent with the political substance of legislative redistricting discussed here. Our empirical analysis also succeeds by using more powerful methods, more accurate information about more redistricting plans, and dozens of times more data than have heretofore been brought to bear on these problems. Our empirical results have important counterintuitive policy implications, since, in total, they imply that the existence of legislative redistricting—and even partisan-controlled gerrymandering—has beneficial effects on American electoral systems, increasing electoral responsiveness and reducing partisan bias.

THE SCHOLARLY DEBATE AND PROPOSED RESOLUTIONS

We shall begin by introducing the scholarly debates, proposing resolutions, and overviewing our empirical results.

Electoral Responsiveness

Electoral responsiveness is the degree to which the partisan composition of the legislature responds to changes in voter preferences. Although closely related concepts exist—including the competitiveness of the electoral system, the probability that an incumbent will lose a reelection bid, the frequency of marginal seats, and the swing ratio—we find electoral responsiveness (which we shall define precisely later) to be the most direct representation of the relevant theoretical concept of interest.[6]

Political scientists have typically taken two contradictory positions about the effect of redistricting on the responsiveness of an electoral system. One set of scholars maintain that partisan and bipartisan redistricting plans reduce electoral responsiveness. For example, Cain writes, "Because incumbents tend to be risk averse—no margin of safety is too much—the result [of a *bipartisan* redistricting plan] is greater electoral inefficiency and more noncompetitive seats" (1985, 321). Mayhew (1971) and Tufte (1973) also argue that *bipartisan* redistricting plans are primarily incumbent protection, hence reducing responsiveness of legislative seats to citizen votes. Owen and Grofman (1988) show theoretically that optimal *partisan* redistricting plans should also produce a less responsive electoral system. A different position has been argued by another group of scholars: "Redistricting has no influence at all on the swing ratio" (Ferejohn 1977; see also Burnham 1974).

This is an important scholarly debate, but neither position is fully consistent with our prior qualitative knowledge. For example, although some incumbents benefit from redistricting, all (or even most) do not. Many of the incumbents of the party not in control of the process will lose support even if they are not actually paired into the same districts. Some will intentionally reduce their general election support in order to avoid a primary. Moreover, because of geographic constraints, redistricting even hurts some incumbents of the party in control. Improving the partisan composition of a district for one incumbent requires modifying the neighboring district boundaries, and neighboring districts are not always conveniently open seats or held by opposition party members. As a result, in addition to interparty competition, redistricting frequently creates intraparty competition among rational officeholders seeking to maximize their probability of reelection: "[The] scrambling of incumbents can have momentous importance for the election that follows the redistricting" (Cain 1985, 331).

How could redistricting have no effect on—or even reduce—electoral responsiveness when it loosens the hold of many incumbents of both parties on their electoral constituencies and reduces their chances of reelection? In fact, our empirical results indicate that both prevailing positions in the literature are incorrect. Redistricting (whether partisan or bipartisan) tends, on average, to *increase* electoral responsiveness (see also Campagna and Grofman 1990; King 1989). Redistricting does this by shaking up the political system and creating high levels of uncertainty for all participants. Moreover, when redistricters draw lines by jointly maximizing the advantages to their party and their incumbents, they create additional uncertainty and also produce a direct increase in responsiveness by attempting to gain partisan advantage by creating more districts with smaller likely victory margins.

Partisan Bias

Partisan bias is the degree to which an electoral system unfairly favors one political party in the translation of statewide (or nationwide) votes into the partisan division of the legislature. Politicians, journalists, some judges, and many political scientists believe that political parties in control of redistricting produce sizable effects on the degree of partisan bias in the electoral system (see Abramowitz 1983; Born 1985; Cranor, Crawley, and Sheele 1989; Erikson 1972; Gopoian and West 1984; Hacker 1963; Niemi and Winsky 1992). This results in important political consequences. For example, Robert Dixon insists, "Apportionment and districting decisions are primary determinants of the quality of representative democracy" (1968, 14). As a state legislator explained to one of us, "Control of redistricting here is worth $50 billion—the value of the state's budget per year for ten years."

In contrast, much recent work in political science has found relatively minor partisan effects of redistricting (Bullock 1975; Campagna and Grofman 1990; Ferejohn 1977; Glazer, Grofman, and Robbins 1987; Scarrow 1982). Cain argues that "even the most egregious partisan gerrymanders do not 'lock-in' one party's control over the state: Districting only affects control of a few seats, and it can be rendered meaningless by large state or national shifts in voting patterns" (1985, 226). Niemi and Jackman conclude that "as in the congressional case, redistricting of state legislatures is less subject to partisan gerrymandering and resulting partisan bias than popular commentary would suggest" (1991, 198).

Thus, one side holds that gerrymanderers draw district lines in order to maximize only their party's seat advantage and have a large and lasting effect, while the other argues that whatever gerrymanderers maximize, they have only a small or transitory effect. Paradoxically, we find that *both* sides in this debate are correct. The disagreement appears to lie in a difference over the precise causal question asked. From the perspective of a close observer of the process and the first group of scholars, redistricting certainly has a partisan "effect", but this effect is defined (implicitly) as the consequence of Democratic-controlled versus bipartisan- or Republican-controlled redistricting. The causal effect of interest to this first group is the difference between bias in the electoral system when redistricting is controlled by Democrats versus Republicans (although obviously only one kind of redistricting is observed at any one time). Any good politician knows the consequences of letting the opposition party draw the district boundaries. We find that the difference here is as predicted: on average, redistricting favors the party that draws the lines more than if the other party were to draw the lines. In fact, the effect is substantial and fades only very gradually over the following 10 years.

The second group of scholars in this debate finds no "effects," or else finds effects that disappear quickly over time. It appears that the causal question asked by this group is distinct from the first, focusing not on the difference between Democratic- and Republican-controlled redistricting but on the difference between the consequences of redistricting versus no redistricting. We find that on average, redistricting (either partisan or bipartisan) actually reduces the degree of bias as compared to no redistricting. Most of the especially effective partisan gerrymanders take a political system severely biased in favor of one party and make it slightly biased in favor of the other, hence reducing the overall bias. This result does not contradict the position of the first group, since partisan plans do favor the party in control compared to bipartisan plans, but they all reduce the overall degree of bias compared to what would have been if no redistricting had occurred. We now turn to the evidence for our claims.

DATA

Our data include every individual-level district election from every state legislative lower house in the United States that elected its members from solely single-member districts in any election from 1968 to 1988. These data span 30 state legislatures, 60 redistrictings (with 1, 2, or 3 per state), 267 statewide elections, and 29,679 district-level elections, in total providing a much wider and more detailed base for comparative empirical analysis than has been previously brought to bear on these problems.[7]

Furthermore, in order to assess the effects of redistricting, we must determine when redistrictings occur and whether each redistricting plan was Democratic-controlled, Republican-controlled, or bipartisan.[8] Unfortunately, adequate information about state redistricting processes have never been compiled. Sources such as Hardy, Heslop, and Anderson 1981 and Cortner 1970 and numerous court cases provide valuable but insufficient information. Previous studies of political gerrymandering either analyze a single case or a few cases in depth (e.g., Cain 1984; King 1989; Scarrow 1982) or use only indirect evidence of partisan control of redistricting (see Morrill 1990 for a partial exception). For example, Erikson (1972), Born (1985), and King and Browning (1987) infer control of redistricting processes indirectly by noting the party that controlled the state legislature and governorship, with special rules to deal with court challenges and other exceptions. (If one party controlled all three, the plan was assumed to have been gerrymandered by that party; if control was split, they concluded the plan was bipartisan.) This inferential procedure has the advantage of being easy to implement and is often correct, but it is misleading in many cases. For example, some state constitutions give control of redistricting to bipartisan commissions, regardless of who controls the government. In other states, the courts have at times implemented the minority party's redistricting plan (on grounds other than political gerrymandering but presumably with the same effect). And in all states, creative maneuvers by politicians, using techniques such as court challenges and legislative impasses, can cause redistricting to occur at times other than immediately following the decennial censuses. Using these indirect methods causes many redistrictings to be missed and many of those not missed to be misclassified.

To avoid problems with existing measures, we conducted an in-depth study of each redistricting process in every state. We mailed a questionnaire to every state legislature, requesting the names and party affiliations of all individuals who participated in the redistricting process, the official and unofficial rules of the apportionment and districting process, copies of the final redistricting bills, and certain district maps. We then interviewed state election officials, state court justices, commission members, attorneys, academics, legislators, and political party officials, as well as looking at many state newspapers and scholarly literature. Throughout, the goal was to gauge the intention, rather than the perceived effect, or publicly stated goal, of a particular redistricting plan. Regardless of whether the redistricting was implemented by a legislature, a governor, a commission, or a court, we categorized each plan by its partisan intention. We finished collecting the redistricting data before calculating any estimates from our electoral data to eliminate possible coder-induced endogeneity (in fact, an early version of our data were used almost three years ago; see Niemi and Jackman 1991). From this information, we identified 60 redistrictings and classified each as Democratic-controlled, Republican-controlled, or bipartisan.[9] The states, years, and classifications of the redistrictings appear in Appendix B.

DEFINING AND ESTIMATING ELECTORAL RESPONSIVENESS AND PARTISAN BIAS

We estimate electoral responsiveness and partisan bias in each state legislature, for each of the 267 election years, using the model described by Gelman and King (1994) and the associated computer program. Although we developed this statistical model to estimate bias and responsiveness in legislative data, it has numerous other applications. This methodology is briefly summarized in Appendix A. For each state and election year, we calculate a point estimate and standard error for electoral responsiveness and partisan bias and the same quantities in the counterfactual situation in which all incumbents suddenly retire; we use these for all subsequent analyses. Results from our numerous auxiliary analyses not reported here, with alternative measures of these and related concepts, strongly support our substantive conclusions described. For each state election in the data set, our estimates of electoral responsiveness and partisan bias, along with the number of seats in the legislative house, appear in Appendix B.

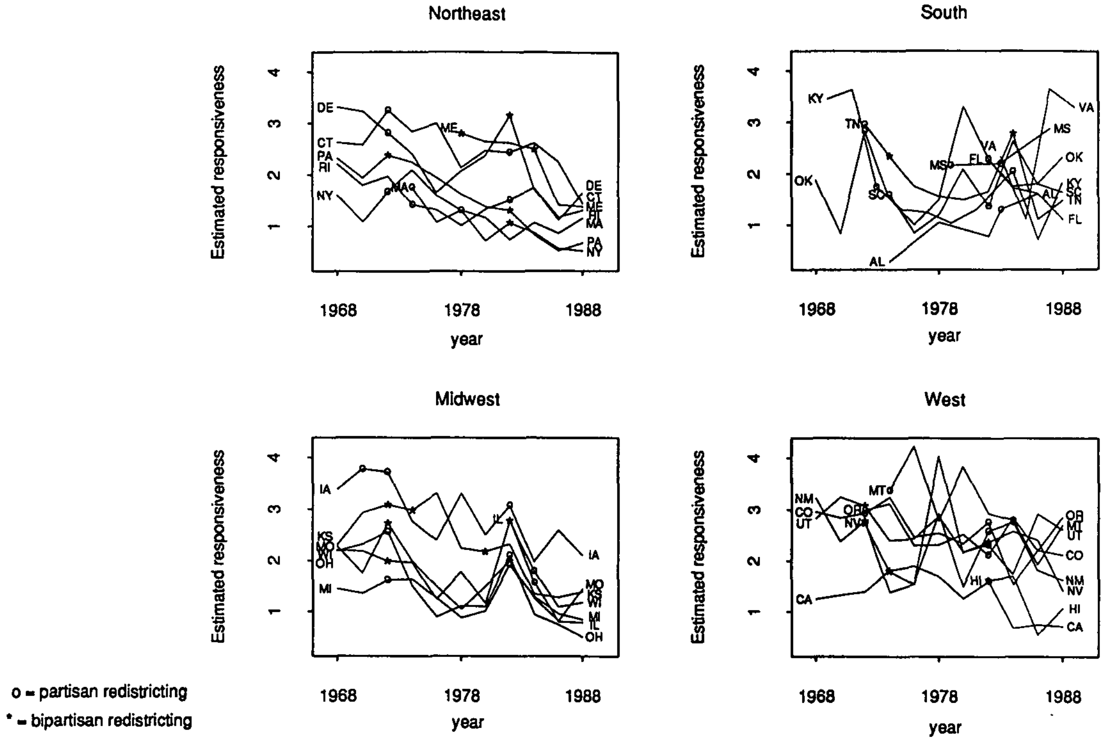
In order to define these concepts more precisely, we define $\bar{v}$ to be the average Democratic proportion of the two-party vote across districts in the state (corrected for uncontested seats; see Gelman and King 1994 for details) and $\bar{s}$ to be the Democratic proportion of the seats in the legislature. We also account for the effects of differential turnout.[10] For each state's electoral system in each election year, we estimate electoral responsiveness and partisan bias. (We take our definition of these concepts from King and Gelman 1991, which generalized the definitions introduced in King and Browning 1987 and King 1989.)

Electoral Responsiveness

We define *electoral responsiveness* as the change in the expected seat proportion given a small change in the vote proportion, from slightly more Democratic than the average district vote to slightly more Republican (see King and Browning 1987; Gelman and King 1994). For present purposes, we use a swing of 1% in each direction from the election outcome: responsiveness is the average difference, $[E(\bar{s}|\bar{v} + .01) - E(\bar{s}|\bar{v} - .01)]$, divided by the vote swing, .02.[11] For example, a value of responsiveness of 1.0 is (in the absence of bias) de facto proportional representation. A value of 2.0 (approximately the average value across all the data we analyze) indicates that a 1% increase in the average district vote share for Democratic candidates statewide will produce a 2% increase in the Democratic share of the state legislature. Scholars of American politics almost uniformly take the normative position that higher values of responsiveness indicate a healthier democracy (e.g., Ferejohn 1977). (In stark contrast, scholars from most other countries prefer proportional representation and therefore lower values of responsiveness nearer 1.0; a valuable

FIGURE 1

Electoral Responsiveness over Time in Each State, by Region



Source: Calculated from district-level electoral data using JudgeIt (Gelman and King 1993). For each state, data are included only for years in which all elections were held in single-member districts.

topic for future research would be to work out the conditions under which each normative standard is most appropriate.) Figure 1 presents a descriptive view of electoral responsiveness over time in each state in our data set. Most states have responsiveness values between 1.0 and 3.0. Except for the South, responsiveness has gradually dropped over time, just as in the U.S. Congress (King and Gelman, 1991). As the Republicans have gained strength in the South, the legislatures of southern states have become more competitive with increasing electoral responsiveness.

Partisan Bias

Partisan bias is the deviation from partisan symmetry when the average district vote is between $\bar{v} = .45$ and $\bar{v} = .55$. For example, if one party is able to translate 55% of the average district vote into 75% of the seats in the legislature, then it would be symmetric for the other party, too, when it receives 55% of the average district vote, to receive 75% of the seats. We define *partisan bias* as the proportion of the seats in the legislature that the Democrats receive over and above what is fair according to this symmetry criterion. A positive bias favors the Democrats, whereas a negative value for bias indicates that the electoral system favors the Republicans. For example, if partisan bias is $-.05$, then the Democrats receive 5% fewer seats in the legislature than they should under the symmetry standard (and the Republicans receive 5% more seats than they should).

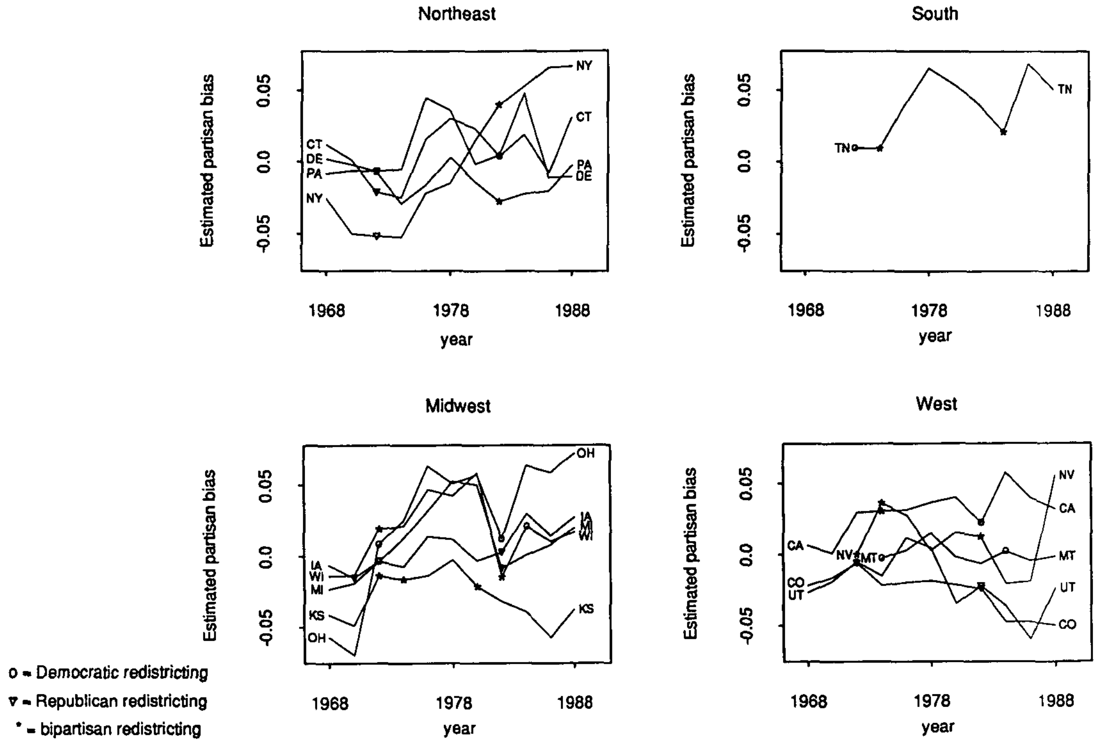
Unlike electoral responsiveness, estimating partisan bias requires imagining what would happen if the minority party were to become the majority party in some future election. Although this is obviously possible in any state, it is extremely unlikely in some. Forecasting what would happen if, for example, the Republicans suddenly won a majority of the Alabama legislature is beyond the scope of any empirical analysis. We therefore limit our analysis to "competitive electoral systems," which we define as states in which each political party managed to garner a majority of seats or votes in at least one election between 1968 to 1988. For the analysis of bias, this reduces our data set to 16 states and 164 elections. Less restrictive definitions of *competitive* do not materially change our substantive results.[12]

Figure 2 displays our estimates of partisan bias over time in each state (only for "competitive electoral systems"). Most of the bias figures are between 5% favoring the Democrats to 5% favoring the Republicans. Partisan bias in these states seems to be trending from favoring the Republicans to favoring the Democrats. This is also true of the U.S. House (see King and Gelman 1991).

Finally, we also estimate what values each of these three quantities would be if, at the start of each

FIGURE 2

Partisan Bias over Time in Each State, by Region



Note: Positive values for estimated partisan bias on the vertical axes in these graphs indicate electoral systems that unfairly favor the Democratic Party; negative values indicate bias in favor of the Republican Party. Calculated from district-level electoral data using JudgeIt (Gelman and King 1993). For each state, data are included only for years in which all elections were held in single-member districts. Only states with competitive electoral systems are included.

election campaign, all incumbents decided not to run. This simulated universal term limitation helps us ascertain the role of incumbents in redistricting. We omit the analogous time series plots of these figures, but to gain an understanding of the changes in incumbency over time, we present Figure 3. For each year in the data set, this figure displays the number of redistrictings in that year by the proportion of incumbents running for election.[13] A line connects the points so one can easily trace the path of the electoral system over time. Two patterns are clearly evident in this graph. First, from 1968 to 1988, the proportion of incumbents has increased from about 50% to nearly 70%, as portrayed in the graph by the line headed steadily upward. Second, the proportion of incumbents clearly decreases during redistricting years. Figure 3 also portrays an important effect of redistricting. Some incumbents are undoubtedly "retired" involuntarily by redistricting plans that eliminate their political base or pair them in districts, forcing them to run against each other. However, many other incumbents probably also retire to avoid the huge political fight of redistricting itself. If a state legislator were thinking of retiring (or running for another office) at some time over the next few elections, then planning an exit to coincide with a redistricting would save a lot of grief, if not political defeat.

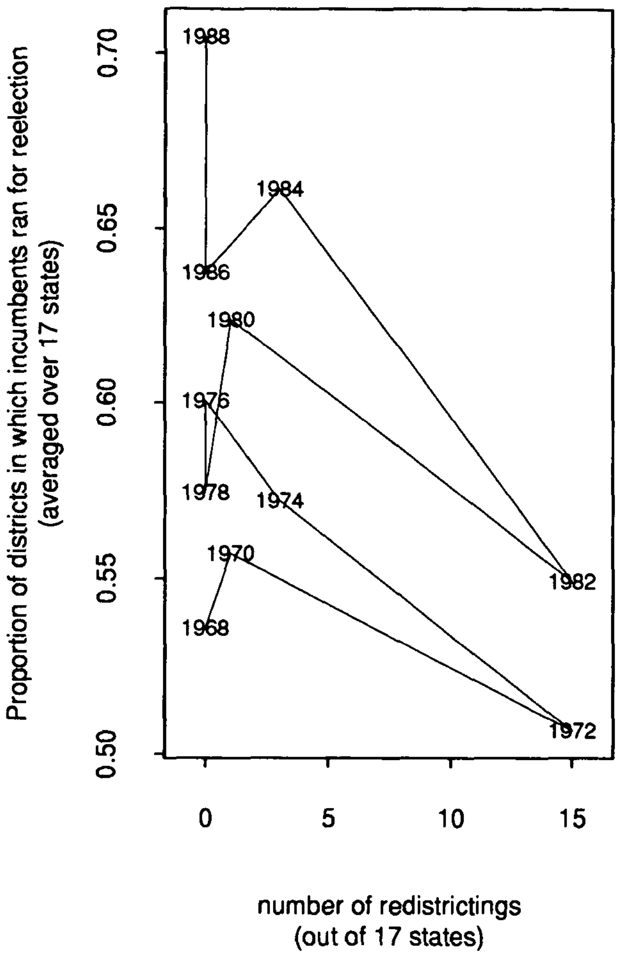
A REGRESSION MODEL

We conduct parallel analyses of electoral responsiveness and partisan bias. Each variable has the state election year as the unit of analysis ($n = 267$ for responsiveness and $n = 164$ for bias). The core analysis involves a straightforward regression model using each of these dependent variables in turn. We present the regression analysis of each numerically and then provide a further interpretation of these regression results with two forms of graphical analysis.

We denote Y_{it} as the dependent variable (electoral responsiveness or partisan bias), minus its mean, for election year t and state i.[14] In addition, we define the following explanatory variables:[15] *redistricting*, R_{it}, is 1 if a redistricting occurred immediately before election year t in state i, and 0 otherwise; *partisan redistricting*, P_{it}, is .5 if a redistricting occurred immediately before election year t in state i and was exclusively controlled by one of the major political parties, $-.5$ if the redistricting was bipartisan (controlled by both parties), and 0 otherwise; *democratic-controlled redistricting*, D_{it}, is 1 if the Democratic party controlled the redistricting immediately before election year t in state i, -1 if the redistricting was Republican-controlled, and 0 otherwise; and *state effects*, S_{it}, are a set of 0/1 dummy variables representing the states.

FIGURE 3

Proportion of Incumbents Versus Number of Redistrictings, over Time (Averaged over 17 States)



Regressions for Electoral Responsiveness. We do not use all the explanatory variables for each regression. To assess the effects of redistricting on electoral responsiveness, we estimate the following linear regression:

$$E(Y_{it}) = \beta_1 R_{it} + \beta_2 (Y_{i,t-1} R_{it}) + \beta_3 P_{it} + \beta_4 Y_{i,t-1} + \gamma S_{it}, \quad \textbf{(1)}$$

where β_1, β_2, β_3, and β_4 are regression coefficients, γ is a vector of regression coefficients, and $(Y_{i,t-1}R_{it})$ is an interaction term calculated by taking the product of the lagged dependent and redistricting variables.[16]

Inclusion of the state variables, S_{it}, is a standard procedure recommended in time series cross-sectional literature, where it is called a "fixed effects model" (Hsiao 1986; Stimson 1985). These variables enable us to pool different states safely, guaranteeing that we are comparing (for example) New York in one year with New York in another, rather than New York in one year with Rhode Island in another. (Including all the state variables also makes the constant term in the regression unnecessary.) From a theoretical perspective, a random effects model might be preferred (e.g., Dempster, Rubin, and Tsutakawa 1981). We repeated all our regressions with random state effects and found no major substantive changes in the results.[17]

As such, estimating the coefficients in γ is important, but the values they take on are not of direct interest; we therefore omit these from our tables. The remaining coefficients are of interest and are interpreted as follows: β_1 is the average effect of any redistricting in increasing Y (responsiveness or bias); β_2 is generally negative, indicating how much larger the effect of redistricting is for small previous values of Y and how much smaller it is for larger values of Y (β_2 is, equivalently, the drop in the persistence in the level of responsiveness between two elections due to a redistricting intervening); β_3 is the additional constant effect of partisan versus bipartisan redistricting over and above the average effect; and β_4 indicates the persistence of Y over time in the absence of redistricting.[18]

Regressions for Partisan Bias. To estimate the effects of redistricting on partisan bias, we change equation 1 only by substituting P_{it} with D_{it}. This changes the interpretation of β_3 to the constant effect of Democratic- versus Republican-controlled redistricting over and above the average effect. The interpretation of the other coefficients does not change.

It should be possible to improve our model if additional data become available, but in the many alternative models and diagnostic tests we tried, we found no evidence to contradict the model in equation 1. We also found the error distribution and autocorrelation structure of the data to be consistent with the time series behavior of the model.[19]

Although we found no evidence of nonlinearities, the empirical independence of the lagged dependent variable and the redistricting variables makes our specification unreliant upon the linearity assumption in estimating our key causal effects. Finally, the substantive results we are about to present were robust across all reasonable specifications that were consistent with the data.

EVIDENCE

We present our empirical results first for electoral responsiveness and then for partisan bias. We conduct the analyses in each of these sections in analogous fashion and explain our procedures in most detail in the first.

Electoral Responsiveness

Our regressions explaining electoral responsiveness appear in Table 1. Column 1 reports the estimated regression effects (with standard errors in parentheses) on the actual level of responsiveness. Column 2 reports estimates for a regression with the same explanatory variables but with a forecast of what

TABLE 1

Effects of Redistricting on Electoral Responsiveness: Regression Estimates

EXPLANATORY VARIABLE (PARAMETER)	ACTUAL	NO INCUMBENTS RUNNING
Redistricting ($\hat{\beta}_1$)	.47 (.08)	.98 (.11)
Redistricting interaction ($\hat{\beta}_2$)	−.17 (.10)	−.20 (.11)
Partisan redistricting ($\hat{\beta}_3$)	−.23 (.16)	−.32 (.21)
Lagged responsiveness ($\hat{\beta}_4$)	.38 (.06)	.17 (.06)
Residual standard deviation ($\hat{\sigma}$)	.51	.64

Note: Entries are coefficients from a weighted least squares regression; standard errors appear in parentheses. N = 237 election years (we dropped the first case in each state so that we could regress on the lagged variable). The state effects, S_{it}, are also included in the regressions but are omitted from the table because their coefficients are not of direct interest.

electoral responsiveness would be under the situation with no incumbents running as the dependent variable. We discuss these coefficients here, followed by a more detailed interpretation in several figures.

The primary effect indicates that on average, redistricting *increases* responsiveness by about half a point (.47 plus or minus the .08 standard error). This is a substantial effect, consistent with our qualitative understanding that redistricting creates uncertainty by shaking up the political system and that redistricters are maximizing competing goals. Table 1 also indicates that the effect on responsiveness in a hypothetical electoral system without incumbents is even larger. Put differently, by adding incumbents into the electoral system and redistricting process, the increase in responsiveness that results from redistricting is lessened. We attribute this effect to incumbents trying to maximize their probability of winning the general election, since our analysis holds constant all the other constraints we have observed.

The "redistricting interaction" coefficient indicates that the effect of redistricting is slightly larger when responsiveness is low, and smaller for higher values of responsiveness. This is a "regression to the mean" that pushes the effects of all types of redistricting toward a common level. The effect is not much larger than its standard error, but our many experiments with auxiliary regressions testing this hypothesis convinces us that this effect, although small, is real. As expected, the effect of partisan versus bipartisan redistricting is small and imprecisely measured. The lag of responsiveness indicates that in nonredistricting years, redistricting has a moderate level of persistence, meaning that it is possible to forecast responsiveness from its previous values (and the state coefficients). Put differently, in the absence of redistricting, electoral responsiveness is moderately persistent across elections. However, when a redistricting occurs between these two elections, most of this persistence vanishes (since $\hat{\beta}_4 + \hat{\beta}_2$ is small). Moreover, virtually all of the persistence of responsiveness across a redistricting is due to the presence of incumbents (since $\hat{\beta}_4 + \hat{\beta}_2$ is approximately 0 for Table 1, column 2).

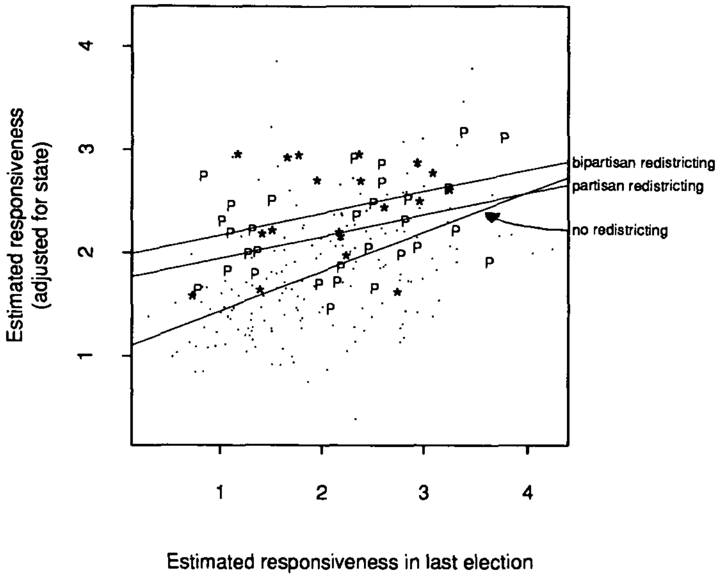
That the persistence of responsiveness across a redistricting is due largely to the incumbents and their districts is consistent with information we gathered in interviews with district-map-makers. That is, those nominally in charge of redistricting, as well as the people who sit at the computers, drawing lines on maps, almost uniformly report that finding satisfactory districts for incumbents is their most important constraint. They also report that the constraint posed by the presence of incumbents is the one of the biggest factors in making their job difficult.

Immediate Effects of Redistricting on Responsiveness. Figures 4 and 5 portray the effects of redistricting on responsiveness graphically, displaying both the data and the regression results in Table 1. Figure 4 illustrates the immediate effects of redistricting by plotting the lagged value of responsiveness by the current value (adjusted for the state effects by subtracting from each value the estimated coefficient for the corresponding state dummy variable). This figure highlights every data point as well as all the regression coefficients; it also emphasizes exactly how the regression summarizes the data in this case. Each of the 237 state elections appears in this plot as a dot (for elections without redistrictings), an asterisk (for years preceded by bipartisan redistrictings), or the letter *P* (for years preceded by partisan redistrictings). For each point, the responsiveness estimated from that statewide election appears on the vertical axis (after being corrected for the appropriate state effect), and the estimated responsiveness in the state's previous election is shown on the horizontal axis. The most important effect in this figure is that most of the points labeled *P* and * are higher on the vertical axis (for a given value of the horizontal axis) than the dots. This shows directly that responsiveness is higher in years following redistricting even after controlling for responsiveness in the previous election (and adjusting for state means).

We also plot the regression lines, fitted to the three types of election years, to help highlight average effects. The coefficient of lagged responsiveness, $\hat{\beta}_4$ = .38, is the slope of the line labeled "no redistricting." The increase in responsiveness due to redistricting is portrayed by the vertical distance between the no-redistricting line and the partisan or bipartisan redistricting lines. The distance between the differently sloped lines at the center of the graph is the average effect of redistricting: $\hat{\beta}_1 - \hat{\beta}_3 = .70$ for bipartisan and $\hat{\beta}_1 + \hat{\beta}_3 = .24$ for partisan redistricting. The difference between the slopes of the regression line is $\hat{\beta}_2 = -.17$. As can be plainly seen, the effect of redistricting is large for small values of lagged responsiveness (at

FIGURE 4

Effect of Redistricting on Electoral Responsiveness, Next Election



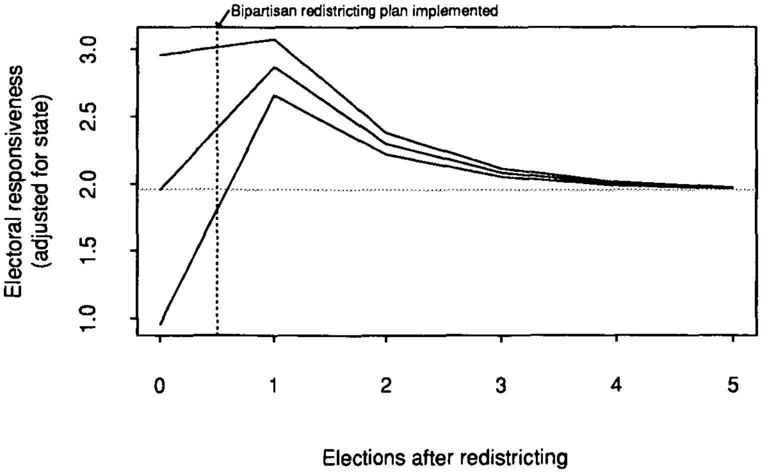
. = no redistricting;
P = partisan redistricting
* = bipartisan redistricting

the left). Or, equivalently, the persistence of responsiveness (as represented by the steepness of the line) is much greater in the absence of redistricting.

Figure 4 helps convey, better than the coefficients in Table 1 can alone, the appropriateness of our regression model for this problem. (For example, one can see the dots on this graph clearly clustered around the no-redistricting line and below the asterisks and *P*s.) Perhaps the most important feature of the graph, confirming that the assumptions of our regression model cannot be rejected by the data, is the absence of any systematic pattern in the data points except those picked up by the regression lines. Finally, Figure 4 shows the unpredictability of redistricting: even after controlling for the lagged responsiveness and state effects, the points on the graph are quite variable. The strong average effects revealed by the regression do not apply to all individual cases.

FIGURE 5

Effect of Redistricting on Electoral Responsiveness, Next Five Elections



Note: The figure shows, for three typical cases, the average effects of a bipartisan plan, relative to what would have happened with no redistricting.
Source: Calculated from our JUDGEIT results and the regression in Table 1.

Longer-Term Effects of Redistricting on Responsiveness. Figure 5 displays the average effects on responsiveness (relative to any trend) for the five elections following a redistricting. The horizontal axis indicates the number of elections since redistricting, with the vertical dashed line indicating the implementation of a bipartisan redistricting plan.[20] To show the differential effects of redistricting, Figure 5 gives three examples, distinguished by the level of responsiveness in the election before redistricting. All three examples show again that, on average, redistricting sharply increases responsiveness in the first election, with the largest effect occurring for states that had low responsiveness before the redistricting. The effects in the graph are calculated from the regression in Table 1 and the implied AR(1) time series model.

Figure 5 shows that the average redistricting effect is very large in the first year, moderate in the second, much smaller in the third, and nonexistent for the fourth and last years. The effect of redistricting in increasing responsiveness does not last until the fifth election, implying that redistricting is unlikely to contribute to longer-term trends. Hence, the existence of many redistrictings increasing responsiveness is consistent with a secular decline in responsiveness over the two decades in our study (see Figure 1). However, this pattern does not mean that the effect of redistricting on responsiveness is unimportant. On the contrary, in the typical state, redistricting occurs after every fifth election, and, as a result, its electoral system benefits from higher levels of responsiveness for roughly half of all elections solely because of redistricting. Redistricting thus boosts the responsiveness of a state electoral system significantly higher than it would otherwise be for about half of all elections. Although any single redistricting does not have permanent effects, the decennial redistricting process is a permanent part of every state's electoral system. As a result, redistricting continually and fundamentally alters the character of representative democracy.

Partisan Bias

In addition to its effect on responsiveness, legislative redistricting has an important effect on the relative fortunes of the political parties. Redistricting can affect the proportion of seats that a party controls in the legislature in two ways. The first, which has been the subject of speculation in the literature but only rarely of empirical analyses, is the effect of redistricting on the average district vote. The second is what we call partisan bias—the effect of redistricting on the allocation of seats between the parties given their average district votes. The ultimate effect of redistricting on the division of parties in the legislature is the sum of these two effects. One can use the seat proportion as the dependent variable in a separate regression to estimate this sum directly. Like most researchers, we prefer a separate estimate of the effects on partisan bias so that we can judge the fairness of the electoral system.

TABLE 2

Effects of Redistricting on Partisan Bias: Regression Estimates

EXPLANATORY VARIABLE (PARAMETER)	ACTUAL	NO INCUMBENTS RUNNING
Redistricting ($\hat{\beta}_1$)	−.012 (.004)	−.012 (.004)
Redistricting interaction ($\hat{\beta}_2$)	−.63 (.12)	−.77 (.16)
Democratic redistricting ($\hat{\beta}_3$)	.010 (.005)	.003 (.006)
Lagged bias ($\hat{\beta}_4$)	.77 (.07)	.55 (.09)
Residual standard deviation ($\hat{\sigma}$)	.014	.015

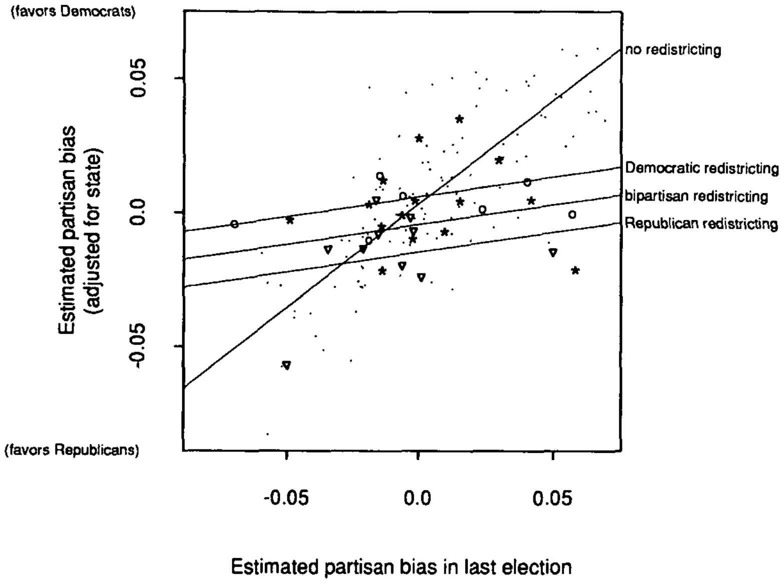
Note: Entries are coefficients from a weighted least squares regression; standard errors appear in parentheses. The dependent variable is estimated partisan bias; as in Figure 2, where this variable is displayed, positive values indicate state electoral systems which favor the Democrats and negative values indicate bias in favor of the Republicans. N = 143 election years (all but the first election in our data set for all states whose electoral systems were "competitive"). The state effects, S_{it}, are also included in these regressions but are omitted from the table because their coefficients are not of direct interest.

Immediate Effects of Redistricting on Partisan Bias. We begin by estimating the regression with bias as the dependent variable, conducted in a manner parallel to that for responsiveness. The results appear in Table 2. Many of the key results here are easier to interpret in conjunction with the graphs in Figures 6 and 7. To begin, note that Figure 6 indicates that partisan bias is rarely greater than about 8% (on both axes) in favor of either party. We believe this is because of the numerous constraints on gerrymanderers, as described in the introduction. We turn now to the effects of redistricting on bias by looking first at the baseline of the effect of lagged bias on current bias in the absence of redistricting. This is portrayed in Figure 6 as a no-redistricting line with the steep slope of $\hat{\beta}_4 = .77$ and indicates that in the absence of redistricting, the level of bias tends to persist much longer than for responsiveness. Because the slope of this line is almost 1, partisan bias changes very little in the absence of redistricting. We therefore expect that whatever effect redistricting has on bias, its effect will take a long time to dissipate.

From this baseline, we can now examine the effects of redistricting. On average, redistricting makes the typical state's electoral system fairer (closer to zero bias) than it would be if redistricting had not occurred. This effect is illustrated in Figure 6 by the asterisks, circles, and triangles (indicating that bipartisan, Democratic, or Republican redistricting, respectively, occurred between the two elections), which are generally much closer to 0 on the vertical axis than the dots (election years without redistricting). This means that state electoral systems are closer to no partisan bias following redistrictings.

FIGURE 6

Effect of Redistricting on Partisan Bias, Next Election



. = no redistricting
○ = Democratic redistricting
∇ = Republican redistricting
* = bipartisan redistricting

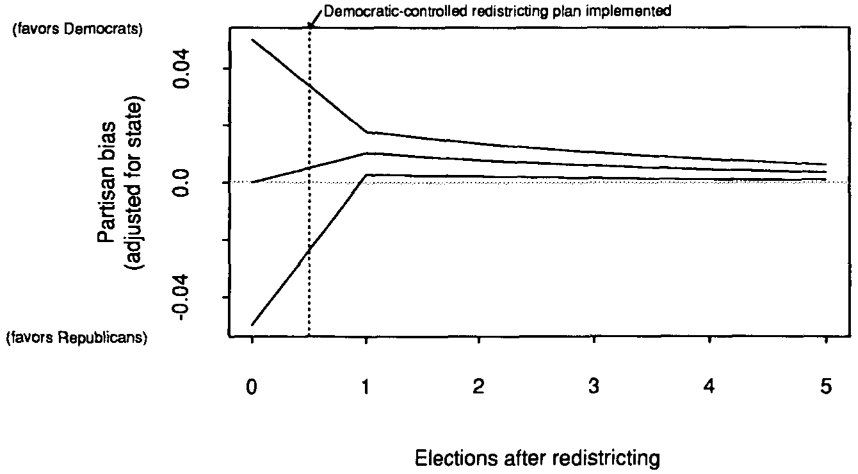
This effect is summarized by the three redistricting lines being much flatter (and closer to 0 on the vertical axis for most of the range of the horizontal axis) than the no-redistricting line. (The difference in the slopes of the redistricting and no-redistricting lines is the redistricting interaction coefficient, $\hat{\beta}_2 = -.63$.)

Thus, no matter how fair or biased the electoral system is to begin with, the typical redistricting plan, whether Democratic, Republican, or bipartisan-controlled, will produce a fairer electoral system.[21] This result is consistent with evidence from individual cases in which the largest effects of redistricting change an existing huge bias in favor of one party to small bias in favor of the other.[22]

The reason that any redistricting reduces bias appears to be the role that redistricting has in shaking up the political system in combination with the many constraints on the mapmakers. Shaking up a system that is effectively constrained to have partisan bias between about ±8% of fairness cannot have an enor-

FIGURE 7

Effect of Redistricting on Partisan Bias, Next Five Elections



Note: The figure shows, for three typical cses, the average effects of a Democratic plan, relative to what would have happened with no redistricting.
Source: Calculated from our JUDGEIT results and the regression in Table 2.

mous effect (although much smaller effects will be quite significant to individual incumbents and political parties). Moreover, if there is already a high level of bias, due to the previous decade's redistricting (or, more likely, to demographic and mobility changes in the population over the decade), any political turmoil will have a higher probability of moving the system toward fairness since there is simply more room to move in that direction.

This powerful role redistricting has in producing fairer electoral systems does *not* imply that Democratic redistrictings produce the same result as Republican or bipartisan ones. To the contrary, the order of the three redistricting lines in Figure 6 (and the effect $\hat{\beta}_3$ in Table 2) indicates that Democratic-controlled redistricting plans typically bias the electoral system toward the Democrats by about 1% (with a standard error of .5%) more than a bipartisan plan. Republican-controlled plans favor the Republicans by about the same amount. Thus, the difference between a Democratic- and Republican-controlled redistricting plan is, on average, an increase in partisan bias of about 2% for the party in control (and a corresponding decrease of about 2% for the other party).[23] The variability of individual results in Figure 6 also indicates that redistricting can have somewhat more or less powerful effects than the average results summarized by the regression lines. With fewer data, less efficient statistical methods, or less careful categorization of redistrictings by partisan control, we may not have been able to distinguish the systematic partisan effects of redistricting from the inherent variability in the system (as seemed to be the case with some existing research).

Finally, Table 2, column 2, displays the effects of redistricting on partisan bias in the hypothetical situation where no incumbents run for reelection. The partisan effect of redistricting, β_3, is statistically (and substantively) indistinguishable from zero in this case. Since incumbents are key actors in either drawing the lines or influencing the line-drawers, this result is fully consistent with the substance of redistricting that we portrayed in the introduction. Incumbents lead the troops into most redistricting battles. That they and their vote-getting abilities would be considered as givens when drawing the district lines is just what we would expect. If, as in our counterfactual condition, many incumbents were unexpectedly defeated, the redistricting might turn out to have very different political consequences.

Thus, partisan-controlled redistricting plans produce electoral systems that favor the party in control more than the opposition party. However, the range of possible outcomes that any redistricter is able to produce, given the complicated constraints and uncertainties, is usually in the neighborhood of near-zero bias. The differences within this neighborhood are still highly significant to the partisans (as we shall further demonstrate), but the overall existence of redistricting constrains bias to within this small and comparatively fair range.

Longer-Term Effects of Redistricting on Partisan Bias. We examine the effects of redistricting on partisan bias over the next five elections with Figure 7, which is based on the regression (and the implied autoregressive time series model with interventions) in Table 2, column 1. This figure is directly analogous to Figure 5, except that the only intervention represented here is a Democratic-controlled redistricting plan. (Estimates for the effects of a Republican-controlled plan are a mirror image of these.) As can be seen clearly by the three lines converging from election year 0 to election year 1, large Republican and even Democratic biases are substantially reduced because of this redistricting. However, the Democrats still produce an electoral system biased in their favor, since all three lines are above zero. Finally, we can see that this immediate effect persists in large measure over the remaining election years before the next redistricting.

Effect of Redistricting on the Average District Vote

We studied the effect of redistricting on votes with the same regression model we developed for bias but using $\bar{v}$ in place of bias as the outcome variable and also including dummy variables for each election year, to control for national swings in the vote. The results indicate that partisan redistrictings increase the proportion of votes for the candidates of the controlling party by an average of about 1% (plus or minus a standard error of .5%) as compared to a bipartisan redistricting. Because responsiveness averaged over all states and elections in our data is about 2.0, this effect on votes typically increases the *seat* proportion for the party controlling the redistricting by about 2% (the 1% effect on votes multiplied by the typical responsiveness of 2.0) as compared to bipartisan control. Thus, the difference in seats between a Democratic- and Republican-controlled redistricting plan is, on average, a substantial 4% of seats. The causal mechanism by which this effect works is probably as follows. A partisan redistricting produces additional districts that the party in control of redistricting is likely to win, as we have demonstrated. As a result, this party finds it easier to field better candidates, which, in turn, produces more votes for those candidates (see Cain 1985; Canon, Schousen, and Sellers 1993).[24]

Unlike the other regression models estimated herein, the regression for votes relies very heavily on the assumption of linearity in order to establish the causal effect of redistricting. The reason is that only in the model for votes are the key causal effects (the redistricting variables) highly correlated with one of the control variables (lagged votes). That is, states with higher Democratic vote proportions in the election before redistricting are more likely to have Democratic-controlled redistricting plans implemented. The correlation with the lagged variable is much weaker in the regressions for bias and responsiveness. The dependence on the linearity assumption of our inference for the effect of redistricting on votes is well within the standards followed throughout the social sciences where key causal variables are often highly correlated with important control variables. As such, we are confident of these estimates. However, we do not proceed with more detailed analyses of this effect (including such features as its persistence over time, as we did for responsiveness), since they would not be as certain as all other results herein, where we are in the fortunate situation of having causal inferences that meet even higher standards (and thus greater certainty) than usual.

Total Effect of Redistricting

Finally, we can add the effect of redistricting on partisan bias (seats given a fixed average district vote) to the effect of redistricting on the division of votes between the parties. This sum gives the total effect of redistricting on the division of seats in the legislature between the parties: if one party controls a redistricting plan, it can expect, on average, to receive approximately 6% of the seats that the other party would have won if it controlled the redistricting. That is, the party drawing the district lines receives, on average, about 6% more seats—and the opposition party 6% fewer seats—than if the opposition party had controlled the mapmaking. Thus, even though redistricting makes the electoral system substantially fairer overall than if there were no redistricting, the difference between Democratic and Republican control over the drawing of district maps is still one that politicians are rightfully concerned about.

We also estimated the effect of redistricting on seats directly, by regressing seats $\bar{s}$ on redistricting type, controlling for state effects, lag seats, the lag-redistricting interaction (as usual), and election year effects. The results of this regression—an effect of 3.2% for the party controlling redistricting, with a standard error of 1.3%—are consistent with our separate analysis of votes and bias. (An effect of 3.2% for partisan redistricting corresponds to 6.4% when comparing Democratic to Republican plans.) In general, we prefer to analyze votes and bias separately, because each is an important consequence of the partisan effect of redistricting.[25] We see the direct regression of seats as a confirmation of our more important results.

CONCLUDING REMARKS ON THE BENEFITS OF REDISTRICTING

As described, our empirical results are consistent with the conflictual and uncertain process of legislative redistricting and the competing goals of redistricters. They also help resolve two important controversies in American politics over the consequences of legislative redistricting for partisan bias and electoral responsiveness. For one, our results demonstrate that contrary to all previous researchers, redistricting in state legislatures has substantially increased electoral responsiveness and kept it higher than it would be otherwise for about half of all elections in each state. The effects of any one redistricting are not permanent, but the decennial redistricting process repeatedly injects the political system with a healthy dose of increased responsiveness. For partisan bias, we have identified a difference in the causal question asked by two groups of researchers, making both sides in this controversy correct to a degree. Our results indicate that partisan and bipartisan redistricting plans reduce bias overall, leading to fairer electoral systems than if there had been no redistricting, but the difference between Democratic-, bipartisan-, and Republican-controlled redistricting plans within this smaller and comparatively fairer region are still politically significant.

We now briefly organize these results from two perspectives. First, individual legislators involved in redistricting can be seen as simultaneously attempting to maximize three partly inconsistent goals: they try to increase the probability of winning or avoiding a primary, winning the general election (conditional on winning the primary), and helping their party win a majority of seats in the legislature. Those responsible for drawing the district lines, whether partisan or bipartisan, always operate in a highly constrained and uncertain environment. The final redistricting plan is usually the result of the process of achieving consensus among incumbents and others, subject to the formal and informal constraints; this process usually produces a plan that weights party advantage heavily.

When incumbents give up votes in order to increase the probability of being in the majority party, responsiveness increases. It also increases when other incumbents retire to avoid the political fight altogether and due merely to changes in district lines and to wholesale increases in uncertainty. Giving up votes in this way also means that Democratic-controlled redistricting plans usually favor the Democrats more than those controlled by the Republicans. However, in order to retain their seats, they do not go too far in trying to achieve this goal. Hence, partisan bias does not favor their party as much as it could. These constraints on partisan bias during redistricting are much more substantial than during the rest of the decade, when changes in demographics, turnout, and the configuration of candidates can cause comparatively larger changes in bias and responsiveness.

A second way to organize these results is to review what they say about the benefits and costs of redistricting for states' representative democracies. The purpose of reapportionment and redistricting is to guarantee that the number of citizens in each district if roughly the same, at least at the start of each decade. Redistricting obviously accomplishes this minimal goal. However, as most political scientists recognize, population equality guarantees almost no form of fairness beyond the numerical equality of population. Even aside from issues raised by counting citizens (rather than voting-age Americans or voters), by representing ethnic minorities fairly, or attempting to ensure that each citizen has an equal say in the policy outcome (which may be impossible to achieve, given such internal legislative rules as seniority on committees), there are the questions of what redistricting does intentionally or unintentionally to the features of our representative democracy that we have discussed here. Allowing state legislators to redistrict opens up the possibility of partisan gerrymanders, incumbent protection plans, and other apparently insidious consequences of the simple task of drawing district lines around equal-sized groups of Americans.

The vast majority of American political scientists have adopted the normative position that healthy representative democracies have low levels of partisan bias and high levels of electoral responsiveness. Our empirical results should make those who support this dominant position yearn for the next redistricting period. The political turmoil created by legislative redistricting creates political renewal. Many of the goals sought by proponents of term limitations are solved by legislative redistricting. Even the reputation of the "egregious" partisan gerrymander has been somewhat rehabilitated: not only does redistricting perform the simple task of getting the numbers right, but redistricting has tended to reduce partisan bias and increase electoral responsiveness.

It is true that bipartisan redistricting produces as high levels of responsiveness and lower levels of partisan bias than partisan-controlled redistricting plans. Moreover, Democratic- and Republican-controlled plans have very different consequences for the parties. One can also still find specific examples of substantial partisan gerrymanders that produce much more partisan bias. These results provide good reason to support a proposal to require bipartisan control of all redistricting processes. If a legislature is incapable of forging a bipartisan agreement, then alternating, or randomly assigned, control of redistricting would also accomplish many of the same benefits. Our results demonstrate that earlier objections to this proposal based on the belief that it will usually create incumbent protection plans (and hence unresponsive electoral systems) are unfounded.

Finally, our results bear directly on the role the courts might choose in resolving partisan gerrymandering claims. The U.S. Supreme Court declared partisan gerrymandering to be justiciable in *Davis* v. *Bandemer* (1986), but it has not yet made clear whether the standards of fairness will be set so that a plaintiff would have a chance of meeting them. On the basis of its recent decisions (e.g., *Voinovich* v. *Quilter* 1993), it seems clear that the Supreme Court would probably prefer not to be involved in partisan redistricting matters, and our results provide them with a clear public policy justification.[26] Individual state redistricting plans sometimes do produce very unfair electoral systems, but on average, recent state redistrictings, even when unattended by the courts, have reduced partisan bias and increased responsiveness. Far from being a scourge on the political system in need of major reforms, legislative redistricting has invigorated American representative democracy.

APPENDIX A: ESTIMATING RESPONSIVENESS AND BIAS

We used the method of Gelman and King (1994), implemented in our computer program called JudgeIt (Gelman and King 1993), to estimate the electoral responsiveness and partisan bias from individual district-level data in each state legislative election. The results of these estimations are displayed in Figures 1 and 2. (JudgeIt and the statistical model underlying our method are described in detail in Gelman and King 1994.) Our method is a straightforward and substantially improved version of our previous method (see Gelman and King 1990; King and Gelman 1991) for defining, estimating, predicting, or evaluating under counterfactual conditions concepts such as seats–votes curves, partisan bias, electoral responsiveness, the expected or predicted vote in each district in a legislature, the probability that a given party will win the seat in each district, the proportion of incumbents or others who will lose their seats, the proportion of women or minority candidates to be elected, the incumbency advantage and other causal effects, the likely effects on the electoral system and district votes of proposed electoral reforms (e.g., term limitations, campaign spending limits, and drawing majority–minority districts), and any other properties of an electoral system that can be defined in terms of vote shares in districts. The method is based on a statistical model that can be applied to virtually any legislature with two major parties and plurality rule elections in districts.

Here we shall briefly outline how our model is applied to estimating responsiveness and partisan bias. As described, we define these quantities based on the seats–votes curve, which in turn we define as the expected proportion of seats, given the average district vote. (These definitions were first given in Gelman and King 1990 and King and Gelman 1991 and are based on King and Browning 1987 and King 1989.) A large literature on the seats–votes curve has accumulated over the past half-century, and several methods have been applied to estimate the function of seats, given votes (or, more precisely, expected seats, given votes). One approach, which uses minimal modeling assumptions, is to estimate the seats–votes curve by a regression of aggregate results from several election years; see, e.g., Tufte 1973). This approach suffers from inefficiency (using aggregate, not district-level, vote information) and, more importantly, cannot be used to measure year-to-year changes, such as we are interested in here. The other traditional approach to estimating the seats–votes curve has been based on the model of "uniform partisan swing" (see, e.g., Butler 1951), a deterministic model that, without modification, applies to no known electoral system.

Our method can be seen as a generalization of uniform partisan swing, with two major improvements. First, we model variability in district election outcomes beyond a uniform statewide swing, thus making the model far more realistic. Second, we include explanatory variables to improve predictions, thus harnessing the full power of regression modeling for district-level data. We also verified each of these assumptions in extensive analyses of all available data on state legislative and congressional elections. We here use past election results, uncontested status, the party of the winner in the previous election, and incumbency as explanatory variables. In addition, we treat uncontested elections in a special way (see Gelman and King 1994).

Although our model applies to any electoral system with two parties or groups of parties, we use the labels "Democratic" and "Republican" to fix ideas more clearly. We assume a legislature comprising n single-member districts and denote v_i as the Democratic proportion of the two-party vote in each district i and v as the set of votes for all districts $(v_1, v_2, \ldots, v_n)$. The votes v will be predicted by k explanatory variables, which can together be written as $n \times k$ matrix, X. We model the district vote outcomes with a random components linear regression of v on X, $v = X\beta + \gamma + \epsilon$, where β is a vector of k parameters that we estimate from each state election, and γ and ϵ are two vectors of independent error terms. The variable ϵ is a traditional random error term; γ is the "random component" error term, which helps correct for the fact that the X variables do not completely describe the state of the electoral system at the start of the campaign due to the omission of relevant variables and measurement error in the variables included. For each district i, the error terms are assigned independent normal distributions, $\gamma_i \sim N(0, \sigma_\gamma^2)$, $\epsilon_i \sim N(0, \sigma_\epsilon^2)$, with variances σ_γ^2 and σ_ϵ^2 that we estimate from several years of election data for each state.

The parameters of this model to be estimated—σ_γ^2, σ_ϵ^2, and β—are not usually of primary interest in evaluating electoral systems and redistricting plans (although β is in some cases of interest in evaluating causal effects). Instead, we define theoretically all the quantities of interest, including the seats–votes curve, responsiveness, and partisan bias, and then compute posterior distributions of all these quantities using Bayesian simulation. The estimates and standard errors of responsiveness and partisan bias used in this study are just the posterior mean and standard deviation of these quantities, estimated separately for each state election.

APPENDIX B: ESTIMATES OF BIAS AND RESPONSIVENESS FOR STATE ELECTIONS

The posterior mean estimates, along with redistricting information, are presented in Table B-1. Posterior standard deviations, as well as the district-level data upon which the estimates were based, are available from the Inter-University Consortium for Political and Social Research in a Class V data set under the authors' names, or from the authors.

TABLE B-1

Estimates of Bias and Responsiveness and Redistricting Information for State Elections

State	Year	Party Controlling Redistricting	Responsiveness	Partisan Bias	Number of Districts
AL	1974		0.29	0.004	105
AL	1978		1.05	0.002	105
AL	1982		0.78	0.017	105
AL	1983	Dem	1.31	0.044	101
AL	1986		1.61	0.073	105
CA	1968		1.26	0.007	78
CA	1970		1.34	0.001	80
CA	1972		1.41	0.030	79
CA	1974	Bipartisan	1.82	0.031	80
CA	1976		1.90	0.032	78
CA	1978		1.71	0.037	80
CA	1980		1.27	0.040	79
CA	1982	Dem	1.61	0.022	78
CA	1984		0.70	0.057	79
CA	1986		0.76	0.040	80
CA	1988		0.72	0.032	80
CO	1968		2.96	−0.021	65
CO	1970		2.85	−0.016	65
CO	1972	Rep	2.93	−0.006	64
CO	1974		3.25	−0.022	65
CO	1976		2.48	−0.019	65
CO	1978		2.56	−0.018	65
CO	1980		2.35	−0.021	65
CO	1982	Rep	2.77	−0.024	65
CO	1984		1.55	−0.047	65
CO	1986		2.21	−0.047	65
CO	1988		2.13	−0.050	65
CT	1968		2.62	0.012	177
CT	1970		2.59	0.001	177
CT	1972	Rep	3.25	−0.021	151
CT	1974		2.83	−0.025	151
CT	1976		2.99	0.016	151
CT	1978		2.14	0.031	151
CT	1980		2.46	0.024	151
CT	1982	Dem	2.43	0.004	151
CT	1984		2.61	0.019	151
CT	1986		2.26	−0.008	151
CT	1988		1.45	0.032	151
DE	1968		3.31	0.002	39
DE	1970		3.23	−0.002	39
DE	1972	Rep	2.81	−0.007	41
DE	1974		2.40	−0.005	41
DE	1976		1.65	0.045	41
DE	1978		2.07	0.037	41
DE	1980		2.37	−0.001	41
DE	1982	Bipartisan	3.15	0.005	41
DE	1984		1.72	0.048	41
DE	1986		1.13	−0.011	41
DE	1988		1.65	−0.010	41
FL	1982	Dem	2.28	0.017	120
FL	1984		1.73	0.076	120
FL	1986		1.63	0.050	120
FL	1988		1.11	0.068	120
HI	1982	Bipartisan	1.63	0.027	51
HI	1984		1.71	0.051	51
HI	1986		0.56	0.132	51
HI	1988		1.07	0.063	50
IL	1982		2.82	0.001	118
IL	1984	Dem	1.58	0.035	118
IL	1986		0.81	0.043	118
IL	1988		0.80	0.038	118
IA	1968		3.39	−0.006	124
IA	1970	Rep	3.77	−0.015	100
IA	1972	Rep	3.72	−0.003	100
IA	1974		2.77	−0.007	100
IA	1976		2.40	0.014	100
IA	1978		3.31	0.012	100
IA	1980		2.51	−0.003	100
IA	1982	Rep.	3.08	0.003	100
IA	1984		1.98	0.030	100
IA	1986		2.60	0.014	100
IA	1988		2.12	0.028	100
KS	1968		2.31	−0.041	125
KS	1970		2.93	−0.049	125
KS	1972	Bipartisan	3.08	−0.014	125
KS	1974	Bipartisan	2.98	−0.016	125
KS	1976		3.31	−0.014	125
KS	1978		2.25	−0.002	125
KS	1980	Bipartisan	2.18	−0.021	125
KS	1982		2.33	−0.032	125
KS	1984		1.35	−0.039	125
KS	1986		1.29	−0.057	125
KS	1988		1.40	−0.038	125
KY	1969		3.46	0.005	89
KY	1971		3.63	0.009	78
KY	1973	Dem	1.74	0.025	100
KY	1975		1.30	0.066	100
KY	1977		1.25	0.072	100
KY	1979		1.03	0.086	100
KY	1981		1.31	0.081	100
KY	1984	Dem	2.05	0.034	100
KY	1986		0.71	0.109	100
KY	1988		1.82	0.070	99
ME	1978	Bipartisan	2.79	−0.005	150
ME	1980		2.64	0.007	151
ME	1982		2.62	0.011	151
ME	1984	Bipartisan	2.49	0.011	151
ME	1986		1.43	0.028	151
ME	1988		1.39	0.041	151
MA	1974	Dem	1.76	0.034	237
MA	1976		1.07	0.076	236
MA	1978	Dem	1.31	0.052	156
MA	1980		1.17	0.076	158
MA	1982		0.75	0.104	160
MA	1984		1.07	0.092	159
MA	1986		0.87	0.097	159
MA	1988		1.16	0.079	160
MI	1968		1.46	−0.023	110
MI	1970		1.37	−0.019	110
MI	1972	Dem	1.63	−0.004	110
MI	1974		1.65	0.013	110
MI	1976		1.27	0.032	110
MI	1978		0.88	0.053	110
MI	1980		1.02	0.050	110
MI	1982	Rep	1.93	−0.008	110
MI	1984		1.30	0.001	110
MI	1986		0.96	0.008	110
MI	1988		0.85	0.020	110
MS	1979	Dem	2.16	0.001	121
MS	1983	Dem	2.20	0.001	119
MS	1987		2.87	0.003	113
MO	1968		2.28	0.011	163
MO	1970		1.76	0.020	163
MO	1972	Bipartisan	2.73	0.004	163
MO	1974		2.00	0.026	162
MO	1976		1.50	0.052	162
MO	1978		1.04	0.070	163
MO	1980		1.51	0.059	163
MO	1982	Bipartisan	2.00	0.021	161
MO	1984		1.26	0.076	162
MO	1986		0.82	0.086	163
MO	1988		1.45	0.063	163
MT	1974	Dem	3.38	−0.002	100
MT	1978		2.80	0.015	100

TABLE B-1

State	Year	Party Controlling Redistricting	Respon-siveness	Partisan Bias	Number of Districts
MT	1980		3.84	−0.002	100
MT	1982		2.94	−0.006	100
MT	1984	Dem	2.81	0.002	100
MT	1986		1.94	−0.004	100
MT	1988		2.71	−0.001	100
NV	1972	Bipartisan	2.75	0.000	40
NV	1974	Bipartisan	1.80	0.036	40
NV	1976		1.55	0.028	38
NV	1978		4.04	0.003	37
NV	1980		2.18	0.016	36
NV	1982	Bipartisan	2.38	0.013	40
NV	1984		2.58	−0.020	41
NV	1986		2.42	−0.019	41
NV	1988		1.43	0.055	42
NM	1968		3.22	0.013	70
NM	1970		2.39	0.034	70
NM	1972	Bipartisan	2.79	0.034	70
NM	1974		1.40	0.067	70
NM	1976		1.56	0.072	70
NM	1978		2.93	0.035	70
NM	1980		1.51	0.034	70
NM	1982	Dem	2.59	0.002	70
NM	1984	Dem	2.77	0.025	70
NM	1986		1.82	0.045	70
NM	1988		1.64	0.059	70
NY	1968		1.61	−0.026	150
NY	1970		1.10	−0.050	149
NY	1972	Rep	1.68	−0.052	150
NY	1974		2.09	−0.052	150
NY	1976		1.60	−0.022	150
NY	1978		1.30	−0.015	150
NY	1980		0.72	0.015	150
NY	1982	Bipartisan	1.07	0.041	150
NY	1984		0.87	0.053	150
NY	1986		0.57	0.066	148
NY	1988		0.52	0.067	150
OH	1968		2.19	−0.057	99
OH	1970		2.32	−0.070	99
OH	1972	Dem	2.57	0.009	99
OH	1974		1.51	0.025	99
OH	1976		0.89	0.063	99
OH	1978		1.11	0.051	99
OH	1980		1.10	0.057	99
OH	1982	Dem	2.11	0.012	99
OH	1984		0.95	0.063	99
OH	1986		0.76	0.058	99
OH	1988		0.51	0.072	99
OK	1968		1.87	0.050	99
OK	1970		0.83	0.096	99
OK	1972	Dem	2.82	0.017	101
OK	1974		1.51	0.067	101
OK	1976		1.00	0.091	101
OK	1978		1.50	0.079	101
OK	1980		3.30	0.035	101
OK	1982	Dem	2.29	0.031	101
OK	1984		1.76	0.099	101
OK	1986		1.80	0.055	101
OK	1988		2.32	0.042	101
OR	1972	Rep	3.00	−0.007	60
OR	1974		3.12	0.009	60
OR	1976		2.31	0.030	60
OR	1978		2.33	0.024	60
OR	1980		2.53	0.010	60
OR	1982	Rep	2.13	0.017	60
OR	1984		2.85	0.008	60
OR	1988		2.86	−0.017	60
PA	1968		2.32	−0.008	190
PA	1970		1.95	−0.006	196
PA	1972	Bipartisan	2.38	−0.007	197
PA	1974		2.25	−0.029	197
PA	1976		1.94	−0.016	192
PA	1978		1.63	0.003	201
PA	1980		1.39	−0.014	185
PA	1982	Bipartisan	1.31	−0.028	203
PA	1984		0.83	−0.022	184
PA	1986		0.53	−0.020	190
PA	1988		0.67	−0.003	189
RI	1968		2.21	0.017	100
RI	1970		1.81	0.036	100
RI	1972		1.98	0.040	100
RI	1974	Dem	1.43	0.032	100
RI	1976		1.31	0.048	100
RI	1978		1.01	0.078	100
RI	1980		1.34	0.056	100
RI	1982	Dem	1.53	0.040	100
RI	1984		1.76	0.072	100
RI	1986		1.18	0.061	100
RI	1988		1.32	0.065	100
SC	1974	Dem	1.59	0.015	124
SC	1976		0.84	0.043	124
SC	1978		1.20	0.050	124
SC	1980		2.09	0.041	124
SC	1982	Dem	1.37	0.041	123
SC	1984		2.63	0.072	124
SC	1986		1.81	0.072	123
SC	1988		1.65	0.076	123
TN	1972	Dem	2.96	0.010	99
TN	1974	Bipartisan	2.34	0.009	97
TN	1976		1.76	0.040	94
TN	1978		1.56	0.066	98
TN	1980		1.50	0.055	98
TN	1982		1.66	0.042	98
TN	1984	Bipartisan	2.77	0.021	99
TN	1986		1.11	0.069	99
TN	1988		1.49	0.051	99
UT	1968		2.83	−0.026	69
UT	1970		3.24	−0.019	68
UT	1972	Bipartisan	3.08	−0.005	75
UT	1974		2.41	−0.015	75
UT	1976		2.44	0.012	75
UT	1978		2.88	0.005	75
UT	1980		2.19	−0.034	75
UT	1982	Rep	2.32	−0.022	75
UT	1984		1.75	−0.036	75
UT	1986		2.93	−0.060	75
UT	1988		2.62	−0.024	75
VA	1983		2.34	0.033	99
VA	1985		1.11	0.080	98
VA	1987		3.66	0.021	99
VA	1989		3.29	0.023	98
WI	1968		2.21	−0.014	100
WI	1970		2.19	−0.013	100
WI	1972	Bipartisan	1.99	0.019	99
WI	1974		1.97	0.021	99
WI	1976		1.25	0.047	99
WI	1978		1.79	0.043	99
WI	1980		1.17	0.058	97
WI	1982	Bipartisan	2.78	−0.015	99
WI	1984	Dem	1.80	0.021	99
WI	1986		1.09	0.011	99
WI	1988		1.18	0.017	99

Note: Electoral responsiveness and partisan bias are estimates computed using the JudgeIt program, as outlined in Appendix A. Responsiveness is the expected change in Democratic proportion of seats per change in average Democratic proportion of votes in a state; for example, a responsiveness of 2 means that a swing of 1% in average district vote will cause (on average) a 2% swing in seats. Bias is the expected average difference between the Democratic and Republican proportions of seats when their proportions of the vote range from .45 to .55; for example, a −2% bias roughly corresponds to the Democrats receiving 48% of the seats from 50% of the vote. In our main analysis, we only use the bias figures for states with competitive elections, as discussed in the text.

Notes

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1. We learned of this story from telephone interviews with justices, politicians, and civil servants in Michigan, while collecting our data.

2. In private conversations with us, members of Congress often volunteer their strong support for increasing the size of Congress so that no state would lose a member at redistricting time. However, they also believe that such a proposal would not stand a chance of passing because it seems so self-serving.

3. For example, we observed several white Democratic legislators in one state succeed in drawing many black voters out of their districts. Because blacks vote overwhelming for the Democrats, this action reduced these legislators' likely general election vote margins, if they got to the general election. However, since voters often prefer to be represented by others from their ethnic group, these new district lines may have increased these white legislators' probabilities of winning or avoiding a primary election.

4. During the redistricting process in one state, we spoke to an incumbent whose new district would have probably given him about 75% of the vote, almost exactly what he had before the redistricting. Yet he went to great extents to oppose the new plan because the opposition party, which controlled the redistricting, had, as he said, "ruined his life." It previously took him under an hour to drive to anywhere in his district, but the new district would be stretched almost all the way across the state. In addition, all four of his district offices and even his childrens' schools were drawn out of the district.

5. Even trying to improve their party's chance of winning a majority of seats is in the narrow self-interest of incumbent legislators, since majority party members in most states have more staff, are chairs of committees, gain additional visibility, and are better able to accomplish their policy goals and satisfy their constituents. As one legislator explained it to us, being in the majority party is also "a lot more fun."

6. We have applied the methods described herein to other measures of electoral competitiveness and found only trivial substantive differences from the results reported in the text using our responsiveness measure.

7. We have checked a sample of these ICPSR data with our own data coded directly from the blue books published by several state governments. We found a few errors (and reported them to the ICPSR), but overall these data are remarkably clean and far more reliable than, for example, the ICPSR collections of U.S. House and Senate data.

8. We do not distinguish between bipartisan and nonpartisan redistricting plans. In most states, it is difficult or impossible to do so. Some previous analyses also used a separate category for court-imposed plans, but many courts (especially state courts) are widely known to be very partisan.

9. Errors in this variable, if any, are almost certainly unrelated to other variables in our analysis.

10. We studied the effects of differential turnout across districts on our estimates by repeating all analyses after substituting the statewide vote for the two parties (i.e., the total votes for the Democratic candidates cast statewide as a proportion of major party votes) in place of the average district vote, $\bar{v}$. (For a discussion of the role of turnout in these two definitions of statewide vote, see Ansolabehere, Brady, and Fiorina 1988.) Although the levels and patterns of responsiveness and bias did change in some states, the effects of redistricting on these quantities were not materially different from those based on $\bar{v}$. That is, in the ensuing analyses, Figures 1 and 2 changed in some ways, but Figures 4–7 (and Tables 1 and 2) changed in only substantively trivial ways.

11. Using average electoral responsiveness measured at $\bar{v}$ between .45 and .55 (see Gelman and King 1994) gives very similar results when applied to the present analysis.

12. We also analyzed responsiveness for these subsets of data and found no relevant changes.

13. To maintain comparability, each point on the figure is averaged over only those 17 states that are in our data set for all 20 years.

14. Subtracting the mean from the dependent variable does not affect any substantive results, but it allows the coefficients of redistricting to be interpreted as effects for the average case.

15. The specific codes we use to represent the dummy variables make interpretation convenient but are not otherwise required or consequential.

16. To improve the estimation, we use weighted least squares with the estimated standard error of each observation (our estimated value for responsiveness), pooled within each state, to compute the weight. This is a standard procedure in general and in this literature (see King 1991; King and Browning 1987).

17. The random-effects model altered the regression results in three ways: (1) most of the standard errors decreased slightly, (2) the estimated state effects decreased, and (3) the estimated coefficients of the lagged variables increased. All these changes could be considered as improvements in the model fit, and all are consistent with the theory of random-effects models. However, we used the simpler linear regression model, because the estimated causal effects of interest did not change materially.

18. Another way to interpret equation 1 is by noting that the effect of R_{it} (redistricting) on Y_{it} (responsiveness or bias) is $(\beta_1 + \beta_2 Y_{i,t-1})$, or, equivalently, the effect of $Y_{i,t-1}$ (last election's responsiveness or bias) on Y_{it} is $(\beta_4 + \beta_2 R_{it})$.

19. Equation 1 is a form of an AR(1) time series model. Conditional on the first lag, which is estimated, the model implies a geometric decline in the coefficients for subsequent lags. To evaluate this assumption, we estimated separate regressions with lags 1, 2, 3, and 4 of redistricting, omitting cases with intervening redistrictings. In all cases, we could not reject the assumption that the correlations followed the AR(1) pattern. For example, the theoretical and estimated lag coefficients for responsiveness are as follows:

Lags	Theoretical	Estimated (s.e.)
1	.40	.40 (.06)
2	.16	.15 (.08)
3	.06	.07 (.14)
4	.03	.22 (.33)

They decline as expected, consistent with the theoretical values based on the lag 1 coefficient of .40. (Note that the estimated coefficients at lags 3 and 4 are smaller than their standard errors.) Analysis of partisan bias gave similar results.

20. In the vast majority of states five elections, at most, are held under any redistricting plan. Since the estimated difference between the effects of bipartisan and partisan plans on responsiveness is small, we omit an analogous figures for partisan plans; it looks very similar to Figure 5.

21. Another way to look at this is with descriptive statistics: the average absolute value of bias in years following redistricting was .016, compared to an average absolute value of .028 in nonredistricting years.

22. The biggest example of this—indeed, the largest effect of redistricting ever noted in the academic literature to our knowledge—is the Ohio State Legislature in 1972. See Figure 2 and the more detailed analyses in Gelman and King 1990.

23. We find no evidence of a difference between the aver-

age absolute size of an effect created by Democratic and Republican mapmakers.

24. We find little evidence that redistricting plans typically have an effect by selective placement of nonvoters.

25. In addition, as noted, the causal inference for bias is especially reliable because lagged bias, unlike lagged seats and votes, does not correlate strongly with the type of redistricting.

26. The Supreme Court wrote in *Voinovich* v. *Quilter* (1993):

> Time and again we have emphasized that "reapportionment is primarily the duty and responsibility of the State through its legislature or other body, rather than of a federal court." *Growe* v. *Emison*, 507 U.S. (1993), supra, (quoting *Chapman* v. *Meier*, 420 U.S. 1, 27 [1975]). Accord, *Connor* v. *Finch*, 431 U.S. 407, 414 (1977) ("We have repeatedly emphasized that 'legislative reapportionment is primarily a matter for legislative consideration and determination' " [quoting *Reynolds* v. *Sims*, 377 U.S. 533, 586 {1964}].)

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POWER AND INFLUENCE IN STATE LEGISLATIVE POLICYMAKING: THE INTERACTION OF GENDER AND POSITION IN COMMITTEE HEARING DEBATES

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There is an implicit assumption behind advocating for more minorities or women in elected office, namely, that these officials will bring a new power and influence to their underrepresented groups. However, for women, this idealized viewpoint ignores the social dynamics that subordinate women's words and actions even in "well-balanced" male and female group interactions. Using transcribed verbatim transcripts of 12 state legislative committee hearings, this research analyzes the conversational dynamics of committee members, witnesses, chairs, and sponsors. Sex differences among committee members are highly significant, even after accounting for political factors and structural features of the hearing. In addition, male and female chairs do not conduct hearings in the same way, and these differences affect the behavior of witnesses and committee members. The findings suggest that as the proportion of women increases in a legislative body, men become more verbally aggressive and controlling of the hearing. Women legislators may be seriously disadvantaged and unable to participate equally in legislative policymaking in committee hearings.

The entry of more and more women into the realm of legislative politics suggests that their increased numbers will offer women equally increased opportunities to influence policymaking. Once a group reaches a certain size (a certain "critical mass"), then its position, power, and influence will increase dramatically. For example, scholars have theorized that through numerical gains, the negative effects of tokenism, which serve to marginalize women in an institution, will fade and that feminist women elected to office will no longer hide or subsume their agendas (Carroll 1985; Kanter 1977). Recent empirical research provides some evidence that women's proportional gains translate into policy gains, as women legislators are beginning to prioritize and pursue topics of special interest to women, such as family policies and women's rights (Reingold 1992; Saint-Germain 1989; Thomas 1989, 1991; Thomas and Welch 1991). Over time, women have moved into leadership positions, and early research in this area indicates that women appear to have a different leadership style than men (Dodson and Carroll 1991; Flammang 1985; Rosener 1990).

Women, even after their 1992 electoral gains, still remain a small minority in the U.S. Congress (10.8% in the House, 7% in the Senate). However, over the last 20 years, local- and state-level offices have proved more permeable for women. In 1993, state legislatures had an average of 20.4% women, ranging from a low of 4.3% in Kentucky to a high of 39.5% in the state of Washington (Center for the American Woman and Politics [hereafter CAWP] 1993a). Given the increasing importance of policymaking at the state level and the springboard effect and policy expertise that state legislative experience provides for many congressional members (Berkman 1993; Francis and Riddlesperger 1982), examining women's participation in policymaking and leadership experiences at the state level provides a snapshot of their current and potential impact.

Of course, as many scholars have shown, state legislatures are not monolithic. Comparisons between one state and another are complicated by political culture, length of the session, salary compensation, staff resources, size of the chamber, number of standing committees, use of subcommittees, degree of party competition, and parliamentary rules—to name but a few important differences (Elazar 1972; Francis 1989; Hedlund 1984; Jewell 1962). Despite these complications, Jewell's (1982) comparative work on all 50 states and Francis's (1989) theoretical models of the states' committee systems provide a baseline for locating where a state fits within the U.S. federal system. But, perhaps more importantly, they have also located commonalities across the diverse set of legislative bodies. For example, Francis found that state legislators spent the majority of their time working with legislation, as opposed to constituency service or legislative oversight. Specifically, committee work and sponsorship were a legislator's main focus, with the former taking more time than the latter due to the greater number of bills that pass through committees and bill introduction deadlines that are set early in the session (Francis 1989; National Conference of State Legislators [hereafter NCSL] 1992; Straayer 1990), which have the practical effect of limiting the number of bills a legislator can prepare to sponsor.

Leadership and party-caucus power notwithstanding, the important decision-making role of standing committees (both full and subcommittees) has been recognized and tested by state and congressional scholars (for recent examples, see Evans 1991a; Francis 1989; Francis and Riddlesperger 1982; Hall 1987).

While structural and personal characteristics of an individual (e.g., leadership position, party affiliation, terms in the legislative chamber, policy goals, constituency concerns, desire to make good policy, and expertise or interest in a particular bill) have been considered important determinants of participation in committee hearings (see, esp. Hall 1987), the effect of a committee member's sex has been completely ignored. With the lack of women in Congress, it is not surprising (or necessarily disappointing) that gender is absent in these models. But even the recent work of state legislative scholars has failed to take into account the role of gender, despite a substantial presence of women in state legislatures over the last decade.

The presumption among these scholars must be that gender simply does not matter. Yet an extensive body of research on gender in a variety of disciplines (e.g., management, psychology, communication, linguistics, sociology, political science) has shown that women are not equally situated with men or adequately captured through models built upon male behavior (see, e.g., Gilligan 1982; Guy 1992; Kanter 1977; Kathlene 1989; Ragins and Sundstrom 1989). Women, whether they be 10%, 20%, or 60% of an organization, work within the larger confines of gendered institutions and socially prescribed roles (Guy and Duerst-Lahti 1992; Zimmer 1988). As Janice Yoder (1991) shows, simply increasing women's presence in the workplace to combat the negative effects of tokenism and bring about gender equality ignores the pervasive sexism in society. Yoder reports that studies on tokenism have found that token men do not experience the same negative consequences as token women (i.e., sexism, rather then group size, produces inequities) and that highly masculinized occupations become more, not less, resistant to rapidly increasing numbers of women, a phenomenon Yoder labels the "intrusiveness" effect.

Yoder's assessment in itself has serious implications for women in politics generally and legislative committee studies specifically. Few social and occupational domains are more masculinized than politics. One does not need a thorough review of American history to recognize that elected offices have belonged almost solely to men (especially at the state and national levels) until just very recently—the last 20 or so years. While on one hand we may bemoan that women are making slow (but steady) increases in state legislatures (less than a 1% gain per year), women's pace, historically understood, could be classified as rapid and "intrusive" in a highly masculinized institution (women's presence in state legislatures has increased fivefold since 1969; see CAWP 1993b). If individual power and influence over policymaking occurs mainly in committees (Francis and Riddlesperger 1982; Hall 1987), women may face additional social barriers not considered within existing models of legislative committee behavior.

In the legislative setting, individuals (and therefore groups) obtain influence primarily through two methods: (1) appointments to powerful positions and (2) assignments to and participation in committees. Theoretically, in a gender-neutral institution, all committee chairs have the power to set the agenda and guide committee discussions. If women have more of an interest in certain types of issues, as research on their legislative priorities and sponsorship of bills indicates (Saint-Germain 1989; Thomas and Welch 1991), then, in an equitable setting, female committee chairs would have opportunities to imprint their concerns while directing hearings. Similarly, if committees are composed of some critical mass of women, then the effects of tokenism and marginalization should be eliminated, allowing women to join freely in the debate on bills. Yet Yoder's work suggests that women chairs will not receive the same respect or have the same influence over committee hearings as men; and the more feminized a committee becomes, the more overt hostility will be directed at women by men.

But it is even more complicated than Yoder's work implies. Other research has found that women do not use or perceive their positions of power like their male counterparts (Blair and Stanley 1991; Dodson and Carroll 1991; Duerst-Lahti and Johnson 1990). Eagly and Johnson's (1990) metaanalysis of experimental and organization research on gender and leadership style found that women tended to lead more democratically, while men tended to be more autocratic. Similar results were found in a legislative setting, where women chairs were more likely to use their position as a facilitator or moderator of committee discussion, rather than as a way to control witness testimony, direct committee discussion, and join in the substantive debates (Kathlene 1990, 1991).[1] Yet it is the latter approaches that are associated with the notion of positional power. That women do not use the position in the same way brings up two important questions: (1) Will gaining access to "powerful" positions result in truly equal influence? and (2) If women use their positions differently, is this due to men's negative reaction to women in power or women's freely chosen redefinition of power?

Finally, with regard to rank-and-file committee members, theoretical and empirical research previously discussed strongly indicates that women and men will not be "equal" participants in group discussions. Therefore, electing more women to legislatures will not automatically change the power balance or the influence in policymaking. *Gender*, as described and applied in this literature, is a complex and interacting construct representing struggles over the use and definition of power, methods of managing conflict and building consensus, paths toward implementing change, and resistance by supporters of the status quo. Gender, like class, is a fundamental category of political analysis, not simply another independent variable representing biological sex (Silverberg 1990).

This research attempts to discover where the barriers—and when the imbalances—occur in one important decision-making setting: the committee hearing. While it is possible that men always dominate verbal interactions in mixed-sex groups, other research indicates that the dynamics of dominance are more complex. Such factors as the positional power of speakers plays an important role in gendered verbal behavior, and it appears that there may be important power and influence differences depending on the topic (or issue) being discussed (Craig and Pitts 1990; Kathlene 1991). Documenting precisely whether and when women are effectively kept out of committee hearing debates and under what circumstances women are successful is important for understanding how the status quo resists change despite numerical gains in elected women representatives.

RESEARCH DESIGN AND METHODOLOGY

All the data were collected from the 1989 Colorado State House during the regular legislative session. Although it was not a full-time legislature, Berkman (1993) ranked the Colorado legislature during the mid-1980s as "more professionalized" (thirteenth of 50) based on its relatively long annual session, legislators' compensation, staff size, and control over federal grants (see also Francis 1989; Jewell 1982).

Four institutional features made Colorado a good choice for examining legislators' verbal interactions in committee hearings. First, while Colorado tends to lean Republican, it has a competitive two-party system thereby making partisan comparisons meaningful (Francis 1989; Jewell 1962). In 1989, Republicans controlled both legislative chambers, 60% of the House, and 69% of the Senate, while the governor's office was Democratic. Second, legislative power is not monopolized; rather, it is shared among the majority party caucus, leadership, and standing committees (Francis 1989). However, Francis also found that legislators in Colorado both preferred and believed that standing committees were the locus of decision making for most bills, with the majority party caucus only taking an active role on bills that reached the House floor. And as of the 1989 session, leadership and caucus power were significantly decreased by the citizens of Colorado through the passage of an amendment the previous November. Dubbed GAVEL for "Give A Vote to Every Legislator," the amendment prevented (1) pocket vetoes by the speaker and committee chairs and (2) binding caucus votes. GAVEL lessened the chair's control to set the committee agenda by forcing all bills through the committee process (Lorch 1991; Straayer 1990). Building coalitions and guiding committee and witness discussions were the main avenues of control left for committee chairs. Theoretically, chairs have the ability effectively to kill bills even since GAVEL by scheduling them late in the session. However, in 1989, only a handful of bills died due to expired deadlines. Relatedly, parliamentary rules regarding speaking behavior (such as time limits, turn limits, or seniority privilege) did not exist for committee hearings.[2] Rather, each chair determined the degree of formality that would be followed (Lorch 1991).

Third, Colorado's relatively small legislative body (n = 65 in the House), the restriction on the number of bills a legislator can introduce in a session, and bill introduction deadlines has helped thwart the pressure to create subcommittees (Francis and Riddlesperger 1982; NCSL 1992; Straayer 1990), thereby increasing the importance of standing committees in policymaking. Fourth, in 1989, Colorado House representatives were assigned to an average of 1.72 standing committees, well within state legislators' "satisfaction threshold" ($n \leq 3$) for the optimal number of committee assignments (Francis 1989). With fewer committee assignments, individual legislators are more likely to have studied the bills assigned to their committee, conversed with their committee colleagues about the legislation, and attended most of their committee's hearings; that is, legislators' scheduling conflicts are reduced with fewer committee assignments (see Francis and Riddlesperger 1982). Under these conditions, rank-and-file committee members should be more active in committee policymaking and less reliant on the chair.

All these conditions point to the importance of standing committees in Colorado state policymaking. Indeed, in the House during the 1989 session, first committee hearings proved to be crucial to the final outcome of the bill. Nearly three-quarters (n = 196) of all House bills that passed out of the first committee (n = 274) eventually became law. Similarly, of the 164 House bills that were killed in 1989, over 50% (n = 86) died in the first House committee hearing, with the remaining bills slowly dropping out at each subsequent stage of the process.[3]

The Colorado legislature has long been among the states with the highest proportion of elected women. During 1989, Colorado ranked fifth among the 50 states in the number of women elected to state government, with 33% (n = 22 out of 65) seated in the House and 20% (n = 7 out of 35) in the Senate (CAWP 1989). For testing gender differences in participation rates at committee hearings, the House committees provided a variety of sex compositions. The percentage of women assigned to standing committees ranged from a low of 9% on the powerful State Affairs Committee to a high of 55% on the Education Committee. Two of the 10 committees of reference were chaired by females; 6 committees had female vice-chairs. Both committees chaired by females also had a female vice-chair. All committees were proportionally representative of the legislature's party distribution, with the committee chairs and vice-chairs selected from the majority Republican party.

Sample Selection of Hearings

Twelve committee hearings were selected from the 68 taped and transcribed hearings gathered.[4] I was interested in selecting a group of bills that were likely to bring out women's voices. All eight family/children bills that were sampled during the 1989 session were selected for this analysis based on the presumption that women were likely to engage in discussions about family and children issues since other research has found that women are more likely to prioritize and sponsor these issues (Saint-Germain 1989; Thomas 1994; Thomas and Welch 1991). Four other nonfamily bills selected on the basis of hearing and sponsor characteristics comparable to the family/children bills (i.e., sponsor's sex, chair's sex, the proportion of women on the committee, and the first committee assigned to the bill) provide for a comparison between family and nonfamily bills.

Half of the bills were sponsored by a female; 5 of the 12 hearings were chaired by a female. It was not unusual for the vice-chair to direct the hearing or to cochair the hearing. Four of the selected hearings were chaired by more than one person. In these hearings, it was the male chair (rather than the female vice-chair) who predominated over most of the hearing (80% or more); therefore, these hearings are coded as male-chaired. The percentage of female legislators at the hearing, including the chair and the sponsor, ranged from 12% to 64%. Eight of the 12 bills passed out of their first committee (the hearing under study in this analysis). Ultimately 5 of the 12 bills became law. Table 1 lists the characteristics for each of the hearings analyzed.

Descriptive Statistics of Sample

The 12 hearings represent a total of 13.2 hours of committee discussion, with a mean length of 66.2 minutes, ranging from 26 to 138 minutes. The 12 hearings have a total of 204 speakers acting in one of four positions during the hearing (e.g., sponsor, chair, committee member, witness). Sixty-five percent of the House members (42 out of 65) were represented in one or more of the six hearings. Thirty-three percent of the selected legislators were in their first term, with 2.5 being the average number of terms in office, and there were no sex differences. The average age of the legislators was 47.0 years, but this differed significantly by sex ($t = 3.69$, $p = .000$), with men having a younger average age (mean = 44.2 years) than women (mean = 50.7 years). Sixty-five percent were Republicans (committee members and chairs combined), representing the party distribution in 1989. Two-thirds of the sponsors in this sample of bills were Republicans, which is similar to the sponsorship by party for all bills introduced in the 1989

TABLE 1

Committee Descriptions and Legislative Outcomes of Selected Bills ($N = 12$)

BILL AND TITLE	ISSUE AREA	ASSIGNED COMMITTEE	SEX OF SPONSOR	SEX OF CHAIR	PERCENT FEMALE[a]	OUTCOME[b]
Family						
1066 Disabled children home care program	Family	HEWI[c]	female	female	12	passed/law
1089 Medical benefits for children	Family	HEWI	female	male	36	passed/law
1193 Encourage opportunity for child care	Family	HEWI	female	male	25	passed/PI[d]
1344 Concerning custody proceedings	Family	Judiciary	female	female	55	PI
1071 Domestic abuse restraining orders	Family	Education	male	female	56	passed/law
1234 Enforcement of support obligations	Family	Judiciary	male	male	44	passed/law
1269 Child care expenses and income tax	Family	Finance	male	male	36	tabled
1339 Reporting child abuse	Family	HEWI	male	male	27	PI
Other						
1105 School dropout prevention act	Education	Education	male	female	64	passed/PI
1118 Adult high school diplomas	Education	Education	female	female	56	passed/lost
1263 Reparation payments and social services	S.Services	Finance	male	male	36	passed/law
1309 Superfund implementation	Environment	HEWI	female	male	36	PI

[a] The percent female at the hearing includes all legislators (committee members, sponsor, chair). The variation shown within the same committee reflects one or more of the following changeable conditions: (1) one or more legislators assigned to the committee did not attend the hearing, (2) the sponsor could be a man or a woman, and (3) the chair of a committee could be either a man or a woman, since chairs and vice-chairs directed hearings.
[b] Outcome of bill in committee hearing in study/Final outcome of bill in the 1989 legislative session.
[c] Health, Environment, Welfare, and Institutions.
[d] Postponed indefinitely.

House. Overall, the sample is representative of both the bills and the legislators across many important characteristics.

Dependent Variables

To determine "who holds the floor" in legislative committee hearings, five dependent variables were tested. The first measure, *percent* TIME ELAPSED, is the percentage of the hearing that has elapsed when the speaker first talks,[5] calculated by dividing the number of words spoken at the hearing prior to a speaker's first utterance by the total number of words spoken at the hearing. The point in the hearing when a speaker enters the committee discussion may be an important measure of influence. Earlier comments and/or questions may direct the other committee members to consider the bill based on the concerns of early speakers. In addition, speaking up early in the hearing may influence subsequent witnesses to modify their testimony in order to address the apparent concerns of the committee at the possible expense of emphasizing other important points. Of course, amendments created in committee (as opposed to sponsor amendments brought to the committee to address perceived deficiencies in the drafted bill)[6] result from discussions in the hearing so that early engagement in committee discussion is likely to both guide further discussions and result in a permanent change of the proposed legislation in the form of amendments.

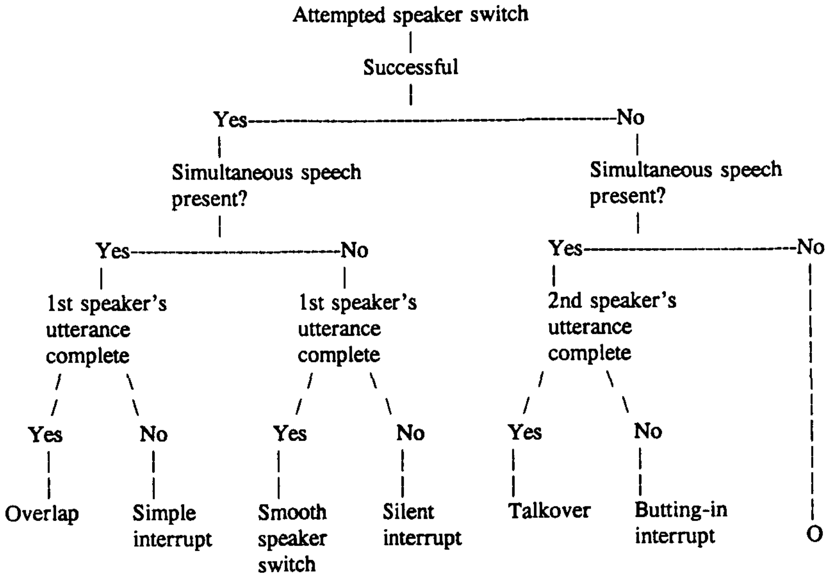
The second two dependent variables, WORDS SPOKEN and TURNS TAKEN, are discrepancy score measures that subtract the expected frequency (the sum of the speaking behavior divided by the number of people present at the hearing) from the actual frequency of a particular speaking behavior. The discrepancy score standardizes for differences in the length of the hearing time to make interhearing speaking behavior comparable. WORDS SPOKEN measures the time a speaker holds the floor. This is an indicator of how vocal a person was during the hearing. Research outside of political settings has found that men talk more than women, whether in mixed- or same-sex groups (Wood 1966). Other studies have documented that males are the more talkative in mixed-sex conversations in the workplace, classroom, and in couple situations (see, e.g., Bernard 1972; Craig and Pitts 1990; Fishman 1978; Swacker 1975; Swann 1988; Woods 1988), and that males talk longer in monologue-type situations (Edelsky 1981). TURNS TAKEN measures how often speakers initiated and engaged in discussion. This is an indicator of how actively involved a person was in the hearing.[7] The interpretation of activity will vary depending upon the positional role of the speakers. When committee members take many turns, it reflects their self-initiation. However, when witnesses take many turns, it reflects the engagement of the legislators (chair, committee members, and, to a lesser extent, the sponsor) through questioning the witness.

The final two dependent variables, *percent* INTERRUPTIONS MADE and *percent* INTERRUPTIONS RECEIVED, measure the different ways committee members gain and lose their speaking turns. Most turns, even in less formal settings than a committee hearing, are gained through "smooth speaker switches," where a person waits until the speaker has completed their utterance. How a person gains their speaking turn is an important indicator of both social norms (i.e., politeness) and power or dominance. Numerous linguistic and sociological studies of mixed-sex group dynamics document the male's verbal aggressiveness. For example, one study found that males interrupting females accounted for 96% of all interruptions (Zimmerman and West 1975). Another study found that men were more likely to talk while a woman was speaking than while a man was speaking (Willis and Williams 1976), though the context and situation of the verbal interaction are important determinants that can reverse the pattern (Murray and Covelli 1988). Interrupting another person reflects the speaker's position of power and dominance, whether it be social or institutionally derived (West and Zimmerman 1977). In committee hearings, there are potentially two conflicting conditions regarding power and dominance. Research on sex differences suggests that men will do a disproportionate amount of interrupting and that women will be interrupted more often than men. But positional power (e.g., chairing the hearing) should mitigate the socially constructed power differences between the sexes. Women chairing committees should not act differently from their male counterparts—or at least, they should be more dominant (i.e., do more interrupting) than either rank-and-file committee members or witnesses (see Kollock, Blumstein, and Schwartz 1985).

An interruption can be successful or unsuccessful. Following Craig and Pitts' (1990) coding scheme, five types of interruptions were identified. Figure 1 diagrams how speaker switches were coded. The five dependent variables examine verbal behavior of legislators and witnesses in committee hearings. While the measures are naturally related to each other (e.g., we would expect that speaking early in the hearing would be positively correlated with speaking more in the hearing), it is the variation among the independent variables that expose the conversational dynamics of a hearing. For example, based on literature previously discussed, men and women may take an equal number of turns, but men may talk longer than women in any given turn. Similarly, interruptions will be related to how much a person talks simply because there is more time to interrupt the person, yet we would expect chairs to make more—but receive less—interruptions by virtue of their position of control. Most of the dependent variables used in this analysis are only moderately correlated (.25 to .50), and all covary in the expected direction. (See Appendix Table A-1.)

FIGURE 1

Classification of Interruptions and Smooth Speaker Switches



Source: Craig and Pitts 1990, 129, based on G. W. Beattie. 1983. *Talk: An Analysis of Speech and Non-Verbal Behavior in Conversation*. Milton Keynes: Open University Press.

Note: Only smooth speaker switches represent taking a turn without interrupting the speaker. Both interruption variables were created by summing together all successful and unsuccessful interruptions (Overlap + Simple Interrupt + Silent Interrupt + Talkover + Butting in) that a speaker made or received. The individual sums were then divided by the total number of turns taken in the hearing to produce percentage scores. While there are theoretically important differences in the type of interruption made, there are too few observations in the data set to analyze the five types separately.

RESULTS

Descriptive Statistics

A total of 116,051 words were spoken with a mean of 9,671 words per hearing. Across all 12 hearings there were 1,747 smooth speakers switches and 195 interruptions. Total interruptions ranged between 4.0% to 26.0% of all speaker switches, with an average of 11.2%. The interruption rate is low in comparison to research findings in conversational dynamics (see Craig and Pitts 1990). No doubt this was due to the formality of the setting. Parliamentary procedures both produce more speaker switches (the chair constantly acknowledges each speaker—55.1 percent of all turns were taken by chairs) and lessen the likelihood of interruptions (speakers generally wait to be acknowledged by the chair). Similar to findings in conversational analysis, most interruptions during the hearings resulted in a successful turn (86.2%) for the person who did the interrupting, further validating the importance of studying who interrupts whom.

Positional Role

Position at the hearing is an important determinate of speaking behavior. Three multivariate regression models of the five dependent variables that demonstrate the power of positional role and speaking behavior are shown in Table 2. In the first model, chairs, committee members, and witnesses all differed significantly from sponsors on the five dependent measures. Chairs took the most turns speaking, while sponsors dominated hearings in speaking time. Sponsors and chairs both made and received more interruptions than committee members or witnesses. The significant differences in elapsed time among the roles are an institutional artifact: sponsors always had the opportunity to summarize their bill verbally at the start of committee hearings; chairs spoke next by virtue of their role as moderator of the hearing; witnesses and committee members were the last to enter into the committee discussion.

The second model adds sex to the equation. The positional patterns found in the first model remain essentially the same. In addition, except for interruptions received, men and women at the hearing also differed significantly from one another. Women entered the discussion later, spoke less, took fewer turns, and made fewer interruptions than men.

The third model includes interaction variables of sex by position. Overall, position continues to distinguish among different speaking behaviors, and female chairs are significantly different than male chairs. Women chairing committees spoke less, took fewer turns, and made fewer interruptions than their male counterparts, suggesting that men and women have different leadership styles. The same gendered pattern holds for female committee members and female witnesses in comparison to their male counterparts, although these differences are not statistically significant.

Since positional role is highly significant, in statis-

TABLE 2

Multivariate Analyses of Speaking Behavior by Position ($N = 204$)

MODELS AND INDEPENDENT VARIABLES[a]	UNSTANDARDIZED REGRESSION COEFFICIENTS				
	% TIME ELAPSED[b]	WORDS SPOKEN[c]	TURNS TAKEN[c]	INTERRUPTS MADE (%)[d]	INTERRUPTS RECEIVED (%)[d]
Model 1					
Chair	.19**	−1791.28****	17.50****	.02	−.02
Comm. member	.54****	−2309.43****	−20.93****	−.20****	−.20****
Witness	.49****	−1749.49****	−19.95****	−.20****	−.20****
Constant	.00	1928.65****	15.92****	.23****	.24****
R^2	.25	.46	.61	.49	.38
Model 2					
Chair	.19**	−1785.30****	17.58****	.02	−.02
Comm. member	.54****	−2326.70****	−21.16****	−.21****	−.20****
Witness	.49****	−1759.81****	−20.09****	−.20****	−.20****
Female	.11****	−203.43****	−2.70***	−.04****	−.02
Constant	−.05	2030.36	17.27****	.25****	.25****
R^2	.28	.47	.62	.52	.39
Model 3					
Main effects					
Chair	.13	−1418.50****	24.29****	.12****	−.04
Comm. member	.46****	−2209.07****	−19.83****	−.18****	−.21****
Witness	.47****	−1619.52****	−19.71****	−.18****	−.22****
Female	.00	69.53	−.06	.02	−.04
Interactions					
Fem. chair	.12	−708.00*	−12.82**	−.18****	.04
Fem. comm. member	.18	−227.56	−2.66	−.05	.01
Fem. witness	.04	−281.43	−.56	−.04	.03
Constant	.00	1893.88	15.95	.23	.25
R^2	.29	.48	.63	.55	.39

[a] Omitted category is the sponsor of the bill.

[b] *Percent time elapsed* measures the percentage of the hearing that has elapsed when the speaker first talks. Smaller percentages indicate early engagement in committee discussion.

[c] *Words spoken* and *turns taken* are discrepancy scores computed as expected frequency minus actual frequency. Negative numbers reflect less activity—positive numbers, more activity—than average. *Words spoken* measures the time a speaker holds the floor, *turns taken*, how often a speaker initiates and engages in discussion.

[d] *Interruptions made* and *interruptions received* are calculated by summing together the number of all the types of interruptions a speaker made (or received) divided by the total number of turns in the hearing. See Figure 1 for a diagram and description of the coding scheme.

*$p \le .15$.
**$p \le .10$.
***$p \le .05$.
****$p \le .01$.

tical terms as well as substantive terms, the analysis will compare speaking behavior *within* each positional role across all 12 hearings.

Committee Members

Personal and Institutional Factors. A look at which group of legislators "hold the floor" in committee hearings most closely mirrors the environment found in linguistic research on conversational dynamics. While there are clear and important differences between informal groups (a group of friends, a set of couples, etc.) and the formality of a public hearing with its political constraints, the committee member plays the most "natural" role of the four types of participants studied in this research. Committee members are relatively free to engage in discussion at any point in time—unlike the witnesses who must wait for their initial turn and subsequent requests for comments. Unlike the sponsor, committee members are not necessarily advocates of a particular bill, although they may be strongly aligned with a particular issue area. Rather than "campaigning" for the bill (as the sponsor does), the committee members' task is to examine the proposed legislation from a multitude of perspectives—personal, professional, and political—that they bring with them. In this way, committee members have the freedom to act more like individuals in a conversation than the witnesses (who are "outsiders" or merely "guests" at the legislative table), the sponsor (whose self-interest is tightly bound to the passage of the bill), or the chair (whose primary formal responsibility is to direct and coordinate the hearing).

Thomas and Welch's (1991) study of legislative activity found that men and women reported similar levels of participation in committee hearings. However, given the research discussed earlier on mixed-sex conversational dynamics, we would expect gender differences between committee members, with men dominating the hearing. Unlike an informal conversation among friends, there are several political and institutional factors that could produce differentiated patterns beyond or instead of gender. Partisan differences seem very likely. We might expect that the minority party (in this case, the Democrats) to be less vocal than the majority Republican party. Or minority party members may be particularly active in committee hearings in order to vocalize their objections to or imprint their concerns on the bill before it reaches the highly partisan dynamics of the House floor debates (Francis 1989; Hall 1987). Or there may be no discernible party differences since committees are viewed by the legislators themselves as one of the least partisan arenas in the legislature (Francis and Riddlesperger 1982).

Other factors could also be important. Hall (1987) found that Congress members' desire to make an imprint on a piece of legislation was strongly related to their participation in committee hearings. Having a personal interest or expertise in the issue area of the bill could explain differences in verbal behavior. Four measures were used to determine a legislator's interest in a bill. A legislator was considered "interested" if (1) the specific bill was mentioned in the personal interview as one of the most important in the session, (2) the issue area of the bill was mentioned among the legislator's top priorities, (3) the legislator sponsored or cosponsored a related bill during the 1989 session, or (4) the legislator's professional experience (e.g., profession, task-force work, community awards) was directly related to the issue area of the bill. Interest in the bill should increase a legislator's activity in the committee hearing.

Finally, legislative expertise may account for greater participation. The number of terms served in the legislature can be used as a general measure of legislative expertise. We would expect newly elected legislators to be inexperienced in state legislative policymaking at a number of different levels (e.g., parliamentary procedures, historical detailed knowledge about failed bills in previous years, and general insider politics) and lack the clout to be included in important off-the-record dealings with legislative leaders, state bureaucrats, and lobbyists. Because the first-term legislators in this research were in the first 60 days of their two-year term, the differences in speaking behavior between the newest members and more seasoned legislators might be very pronounced.[8] Hall (1987) found that freshman status was negatively related to participation in congressional policymaking, and the same may hold true in state legislative committee hearings.

Results of Personal and Institutional Models. Table 3 reports (1) the bivariate regression results of sex on each of the dependent variables and (2) three multivariate regression models. As the bivariate model shows, sex is highly significant on all five measures of speaking behavior. Women, on average, waited until more than two-thirds of the hearing was over before they uttered their first words, while men engaged halfway through the hearing. Men spoke longer and took more turns, and men made and received more interruptions than women committee members. The first multivariate model results indicate that sex continues to be significant across the five measures even after accounting for committee members' interest in the bill, their party affiliation, and their freshman status. In each of the measures, it was the male committee members who dominated the committee hearings.

Other variables are also important in understanding verbal behavior. As predicted, committee members who had an interest in the bill spoke longer and engaged in discussion more frequently. They were also interrupted more often, which follows from their greater verbal activity. Except for making fewer interruptions, minority party status did not affect speaking behavior in committee hearings, supporting Francis's (1989) interviews with state legislators, who reported low levels of partisan politics in committee hearings. Unlike Hall's (1987) findings on congressional representatives, freshman status did not lessen their participation in the hearings. Evans (1991a) found that newcomers to the Senate participated equally with their more senior colleagues, suggesting that there are circumstances that produce greater and lesser inequality based on tenure. Perhaps being a newcomer is more of a disadvantage in a large legislative body, such as the House of Representatives, than in a smaller chamber, such as the Colorado State House (see Evans 1991b).

Additional Committee Hearing Factors and Interactions. Based on recent research on women's legislative priorities and activities (Reingold 1992; Saint-Germain 1989; Thomas 1991; Thomas and Welch 1991), women may be more active participants in hearings that address family issues. If women have a special interest in certain types of legislation, due to their personal experience or belief that they can represent women's interests better than men (Reingold 1992), then female committee members should be more vocal in hearings on family issues.

The sex of the sponsor may have an effect on the committee members' speaking behavior. If women are bringing different views and values to policymaking based on a more needs-based, rather than individual-rights, orientation toward society (Dodson 1991; Dodson and Carroll 1991; Gilligan 1982; Kathlene 1991), then the bills that women sponsor, regardless of issue area, may embody this difference and perhaps strike a responsive cord among the other women committee members.

Chairing a committee is an important position of power because the chair can control much of the hearing dynamics, including in what order committee members will be recognized to speak, which committee members' ideas will be encouraged or cut off, and whether the bill will receive a final committee vote or be tabled (usually resulting in the death of the bill). Given findings in all-female groups that women engage in supportive and cooperative, rather than

TABLE 3

Bivariate and Multivariate Analyses of Speaking Behavior among Committee Members (N = 106)

MODELS AND INDEPENDENT VARIABLES	UNSTANDARDIZED REGRESSION COEFFICIENTS				
	% TIME ELAPSED[a]	WORDS SPOKEN[b]	TURNS TAKEN[b]	INTERRUPTS MADE (%)[c]	INTERRUPTS RECEIVED (%)[c]
Bivariate model:					
Female	.18****	−135.14***	−2.43***	−.04****	−.03***
Constant	.46	56.64	1.00	.05	.05
R^2	.08	.05	.05	.13	.05
Multivariate model 1:					
Female	.19****	−144.49***	−2.66***	−.04****	−.03***
Interest	−.05	153.23***	2.73***	.01	.02*
Minority party	.05	−10.53	−.12	−.02**	−.01
Freshman status	.03	−63.48	−1.34	−.01	−.02
Constant	.45	34.33	.69	.05	.05
R^2	.09	.11	.12	.17	.10
Multivariate model 2:					
Speaker:					
Female	.19****	−144.54***	−2.65***	−.04****	−.04***
Interest	−.06	159.16***	2.84***	.01	.02*
Minority party	.06	−10.88	−.13	−.02*	.00
Freshman status	.02	−60.95	−1.32	−.01	−.02
Hearing:					
Family issue	−.08	26.44	.44	.00	.02
Female sponsor	−.06	−5.87	−.12	.00	.01
Female chair	.11	−25.16	−.41	−.01	.01
Female mass[d]	−.11**	16.12	.23	.00	.01
Constant	.71****	−6.28	.10	.06***	.01
R^2	.13	.11	.12	.18	.15
Multivariate model 3:					
Speaker					
Female	−.28	521.55**	5.09	−.04	.01
Interest	−.05	163.53****	2.89***	.01	.02*
Minority party	.08	−37.04	−.40	−.02**	.00
Freshman status	.00	−44.67	−.80	−.01	−.02
Hearing					
Family	−.02	131.70	1.26	−.01	.04**
Fem. sponsor	−.15*	53.96	−.08	.00	.02
Fem. chair	.10	−5.93	.57	.00	.01
Female mass	−.18***	93.35	1.19	.00	.02
Interactions					
Fem. × Family	−.09	−239.62**	−2.11	.00	−.04
Fem. × Fem. sponsor	.22*	−144.13	−.34	.00	−.02
Fem. × Fem. chair	−.07	−14.13	−1.70	−.01	−.02
Fem. × Fem. mass	.21*	−200.18*	−2.48	.00	.00
Constant	.86****	−269.90*	−2.92	.07***	−.02
R^2	.21	.18	.16	.20	.18

[a] *Percent time elapsed* measures the percentage of the hearing that has elapsed when the speaker first talks. Smaller percentages indicate early engagement in committee discussion.

[b] *Words spoken* and *turns taken* are discrepancy scores computed as expected frequency minus actual frequency. Negative numbers reflect less activity, positive numbers reflect more activity than average. *Words spoken* measures the time a speaker holds the floor, *turns taken,* how often a speaker initiates and engages in discussion.

[c] *Interruptions made* and *interruptions received* are calculated by summing together the number of all the types of interruptions a speaker made (or received) divided by the total number of turns in the hearing. See Figure 1 for a diagram and description of the coding scheme.

[d] *Female mass* represents the proportion of the committee hearing (committee members, sponsor, and chair) that is female. The variable was coded *female tokenism* = 1 (less than 30% female), *female critical mass* = 2 (30%–50% female), *female majority* = 3 (over 50% female).

*$p \le .15$.

**$p \le .10$.

***$p \le .05$.

****$p \le .01$.

competitive, linguistic strategies (Coates 1988), female committee members may be more at ease or more inclined to speak when the chair of the committee is also female. However, other research indicates that even when a token male (or female) is present, the group dynamics change toward a less supportive interaction style (Smith-Lovin and Brody 1989). Therefore, the presence of a female chair may not be enough to bring out female voices. Yet the more democratic leadership style of women—as opposed to men's more autocratic style—may provide the opportunity for more women to participate (see Eagly and Johnson 1990). But being democratic implies an inclusive style that should facilitate all voices, not just women's voices. Thus, men's more aggressive verbal behavior may be even more pronounced under a female chair. Based on these conflicting propositions, female committee members may participate more or less than men when the chair is female.

It may not be enough that the chair or the sponsor is female if the rest of the committee is comprised of mostly males. In order for women to be heard, they may need to comprise some minimal percentage in a committee hearing (Kanter 1977). Conversely, the increased presence of women may result in more verbal activity by men reacting to women's intrusion in the legislative setting (Yoder 1991). The 12 hearings were divided into three groups based on the percentage of women legislators at the hearing (including all the committee members, the sponsor, and the chair).[9] Three bills comprise the *female tokenism* category, in which less than 30% of the legislators were women. Five bills are in the *female critical mass*, where 30% to 50% of the committee is comprised of women. Finally, four bills fall into the category of *female majority*, where, by definition, over 50% are women. If Kanter's proposition holds, female committee members will become more verbally active when more women are present. If Yoder is correct, female committee members will be less active due to men's stepped-up participation to counter the increased proportions of women at the hearing.

Results of Additional Considerations. As multivariate model 2 in Table 3 shows, after controlling for situational factors related to the hearing, the same pattern of male domination found in multivariate model 1 remains. Regardless of whether the chair or the sponsor is a man or a woman or the hearing is on a family issue, female committee members engage later, speak significantly fewer words, take significantly fewer turns, and make and receive fewer interruptions than their male counterparts. However, committee members speak significantly earlier as the proportion of women on the committee increases.

Model 3, which expands on model 2 by adding interaction terms of female committee members by each of the hearing variables, clarifies the differential effect of female presence and positional roles on male and female committee members' participation in the hearing. It is the men, rather than the women, who become significantly more vocal when women comprise greater proportions of the committee, supporting Yoder's "intrusiveness" effect. Similarly, male committee members engaged earlier—and female committee members, later—when the sponsor of the bill was a woman. An examination of the transcript reveals that male committee members asked questions of female sponsors immediately after their introduction and more male committee members engaged early in questioning witnesses that testified for a female-sponsored bill, regardless of the proportion of women at the hearing. Both behaviors tended not to be present when a man sponsored a bill, suggesting that females (but not males) in positions of importance have their ideas scrutinized by rank-and-file men.

While the male committee members engage significantly earlier, they are not speaking or taking more turns under the different situational characteristics of the hearing. The same cannot be said for female committee members. Women not only engage later when there are more women at the hearing, but they also speak fewer words. And women, contrary to the expectation, speak significantly less than men at hearings addressing a family issue, even after controlling for interest in the issue area (which remains significant).

More generally, each of these models, while unable to explain a large proportion of the variance (model 3 explains between 16% and 21%), provide compelling evidence that sex is a powerful explanatory variable. Even when a particular variable is not statistically significant, the direction of the relationship is consistent with female committee members participating less under a variety of controls and interactions, while legislators with a personal or professional interest in the issue participate more.

Witnesses

An examination of witnesses' speaking behavior will help flesh out more of the committee hearing dynamics.

Factors Influencing Witness Speaking Behavior. As noted, witnesses have many constraints on their speaking behavior—most notably that they must wait for the chair to call them forward to speak. Therefore, gender differences in speaking behavior among witnesses may not reflect gender differences between the witnesses but rather differential treatment by the chair (who determines the order of testimony[10] and has the power to limit testimony) and the committee members (who may ask questions of the witnesses). While partisan affiliation is not applicable (at least not determinable), age is unknown, and terms in the legislature cannot be used to measure expertise and political savvy of a witness, other possible factors may be important. Among the most important is the formal affiliation of the witness. Is the witness a lobbyist, a state bureaucrat, an "expert" in a particular field, or a citizen? To most legislators, lobbyists and bureaucrats are likely to be well known, respected, and mostly seen as reliable sources of information (Straayer 1990). However, the familiarity between these witnesses and the legislators may also bring out entrenched ideological differences that cause positive and negative reactions among the legislators. Individual lobbyists and bureaucrats should receive mixed opportunities to speak at the hearing, depending upon the composition of the committee and the issue at hand. Experts may not be personally known by most of the legislators at the hearing, but by virtue of their professional title and experience (e.g., a doctor who specializes in high-risk obstetrics at a hearing on premature infants), experts (as a group) should be given the most opportunity to testify and answer questions. Citizens have no institutional or professional affiliation. Representing only themselves, they are likely to relay a personal story regarding aspects of the bill. Based on research findings that representatives rely on more professionalized sources of information to make policy and voting decisions, unaffiliated witnesses should be given the least opportunity to engage in the committee hearing (see Jewell 1982; Mooney 1991). There are other important characteristics of the witnesses that may cause legislators to treat them differentially. In Colorado, the sponsor of the bill orchestrates the witness testimony, especially in the first committee hearing. Speakers who come to testify at the request of a legislator should receive more opportunity to speak than witnesses who come forward on their own initiative. Relatedly, witnesses who testify in favor of the bill may be treated differently than those who are against it.

Male and female witnesses with different affiliations and roles may not be equally situated with regard to how committee members and chairs respond to them. For example, male experts may be seen as more credible than female experts. Interaction terms will disentangle these potential gendered effects. Finally, the same four hearing variables used to distinguish differences among committee members' verbal behavior are used in multivariate model 2 to test whether witnesses are treated differently under a variety of hearing circumstances related to gender. One additional hearing variable—the number of witnesses at the hearing—is included because the chair knows at the beginning of the hearing how many people have signed up to testify. Given a variety of time constraints the chair works within, the actual number of witnesses signed up for a bill may produce different speaking patterns imposed by the chair. Specifically, the more witnesses signed up, the less time each witness will be allowed to speak and the fewer questions the chair will allow committee members to ask.

Results of Witness Speaking Behavior. As Table 4 shows, the bivariate regression model that tests only sex is not significant for four of the five dependent measures. The chair does not have men and women witnesses come forward to testify earlier or later in the hearing based on their sex. While women witnesses speak less than the expected number of words and men speak more, these differences are not significant, indicting that male and female witnesses are given equal amounts of "floor time." There are no gender differences in the number of turns taken suggesting that committee members (and the chair) are as likely to ask follow-up questions from witnesses of either sex. Interruption patterns do vary by the witnesses' sex. Like their committee member counterparts, female witnesses are significantly less likely to use interruptions to gain a speaking turn than are male witnesses. However, there are no sex differences with regard to witnesses' receiving interruptions, indicating that men and women witnesses are equally likely to be interrupted by the legislators.

Multivariate model 1, based on six witnesses' characteristics, produces several interesting patterns but sex is not significant for the same four dependent measures as found in the bivariate model. Witnesses who come forward on their own, rather than through the sponsor, speak significantly later in the hearing but speak as much—and take as many turns as—the sponsor's witnesses. As hypothesized, "experts" were given the most speaking time, as measured in words spoken and turns taken. Female witnesses continued to make significantly fewer interruptions than male witnesses after controlling for their institutional affiliation, sponsor affiliation, and position on the bill. Interestingly, witnesses who represented themselves made significantly more interruptions, indicating, perhaps, their lack of familiarity with the chair's moderator role.

Multivariate model 2, based on both interaction terms for the witnesses' characteristics and committee hearing characteristics, significantly improves the explained variance for the dependent measures. In hearings on family issues, witnesses spoke significantly earlier and made more interruptions. When the sponsor was a female, witnesses began speaking later in the hearing. This occurred for two reasons: female sponsors tended to give longer introductions to their bills (averaging 1,149 words, as compared to the 806 words for males) and, as noted previously, female sponsors received more questions from male chairs and male committee members during and after their introduction, rather than moving directly to the first witness. The latter interpretation is also borne out in the model where witnesses began speaking earlier in hearings chaired by a female (a woman chair tended to move directly to witness testimony) and were engaged later when there were more female legislators present. We saw earlier that male committee members became more verbally aggressive when more women were present and when the sponsor was a woman. A more extensive interaction model (not shown)[11] found that under a female chair or when the hearing was on a family bill, it was the male witnesses who were making the interruptions, suggesting that increased verbal aggressiveness when women are in power and when women's traditional interests are being discussed is not limited to gendered dynamics among the legislators. Although the model measures witnesses' speaking behavior, these results tell us more about the interaction between witnesses and legislators and how male legislators treat female sponsors at the beginning of the hearing.

The interaction terms in multivariate model 2 produced some interesting gendered patterns that were absent in the main effects model. Overall, the direction of the relationships is consistent with the gendered pattern found among the committee members. Specifically, female witnesses who opposed a bill had

TABLE 4

Bivariate and Multivariate Analyses of Speaking Behavior among Witnesses at Hearing (*N* = 69)

	UNSTANDARDIZED REGRESSION COEFFICIENTS				
MODELS AND INDEPENDENT VARIABLES	% TIME ELAPSED[a]	WORDS SPOKEN[b]	TURNS TAKEN[b]	INTERRUPTS MADE (%)[c]	INTERRUPTS RECEIVED (%)[c]
Bivariate model:					
Female	.04	−144.26	−1.06	−.02**	−.01
Constant	.47	64.81	.46	.04****	.04****
R^2	.01	.01	.01	.04	.01
Multivariate model 1					
Speaker					
Female	.04	−113.74	−.84	−.03***	−.01
Witness affiliation[d]					
Expert	−.08	305.13**	2.36**	.03	−.01
Bureaucrat	−.03	163.86	1.68	.01	.01
Individual	−.01	−223.15	−1.53	.04**	.00
Witness role					
Self-init.	.17***	−147.80	−.60	.01	.02
Against	−.09	31.10	1.09	.01	.02
Constant	.46	38.79	−.15	.03***	.03***
R^2	.10	.11	.11	.11	.09
Multivariate model 2					
Family	−.21***	195.01	.30	.04**	.02
Fem. sponsor	.34****	−262.27	−.16	−.02	.02
Fem. chair	−.37****	270.10	.76	.06**	−.03
Female mass[e]	.14***	9.96	.36	.00	.03**
No. of witnesses	−.09****	55.62	.13	.01	.00
Speaker					
Female	−.05	227.00	1.28	−.01	.00
Witness affiliation					
Expert	−.14	263.21	2.56	.04**	−.01
Bureaucrat	−.02	367.55	3.79***	.03	.04**
Citizen	−.01	150.58	.37	.03	.01
Witness role					
Self-init.	.27****	−213.63	−1.59	−.03	.00
Against	−.13	353.40	3.71**	.03	.07***
Interactions					
Witness affiliation					
Fem. (witness) × expert	.18	263.21	−1.10	−.05	−.02
Fem. (witness) × bureaucrat	.09	−434.20	−5.31**	−.06**	−.08***
Fem. (witness) × citizen	.02	−756.39*	−5.03	−.03	−.01
Witness role					
Fem. (witness) × self-init.	−.31**	439.38	6.67**	.13****	.13****
Fem. (witness) × against	.63****	−1260.58***	−8.81***	−.14****	−.13****
Constant	.80****	−533.65	−2.98	−.05	.07
R^2	.37	.23	.24	.37	.34

[a] *Percent time elapsed* measures the percent of the hearing that has elapsed when the speaker first talks. Smaller percentages indicate early engagement in committee discussion.

[b] *Words spoken* and *turns taken* are discrepancy scores computed as expected frequency minus actual frequency. Negative numbers reflect less activity—positive numbers, more activity—than average. *Words spoken* measures the time a speaker holds the floor, *turns taken*, how often a speaker imitates and engages in discussion.

[c] *Interrupts made* and *interrupts received* are calculated by summing together the number of all the types of interruptions a speaker made (or received) divided by the total number of turns in the hearing. See Figure 1 for a diagram and description of the coding scheme.

[d] Omitted category is lobbyist.

[e] *Female mass* represents the proportion of the committee hearing (committee members, sponsor, and chair) that is female. The variable was coded *female tokenism* = 1 (less than 30% female), *female critical mass* = 2 (30%–50% female), *female majority* = 3 (over 50% female).

*$p \le .15$.

**$p \le .10$.

***$p \le .05$.

****$p \le .01$.

significantly less opportunity, as measured by all the dependent variables, to participate in the hearing than male witnesses who opposed a bill. And female citizens spoke less than male citizens, who were asked more questions by committee members as evidenced in the greater number of turns male witnesses took. While we might expect that citizens and witnesses opposed to bills would receive fewer opportunities to participate in the hearing, this is actually only the case for those who are women. Among the less "politically connected" witnesses, there appears to be a significant gender credibility gap.

However, there is a surprise, too. Female witnesses who testified on their own accord (rather than being asked by the sponsor) spoke earlier in the hearing than their male counterparts. These women also took more turns, made more interruptions, and received more interruptions, but they did not speak significantly more words than the men. The verbal patterns indicate that these women were persistent in trying to take the floor away from other speakers but were unable to hold it long enough to actually say more than the men.

Among the more politicized witnesses, male bureaucrats were asked more questions (they took more turns) but also received more interruptions. Female bureaucrats took significantly fewer turns and made and received fewer interruptions, indicating that they were asked far fewer follow-up questions by the legislators after their initial testimony.

Sex differences among the witnesses—as well as other sex differences related to the sponsor, chair, and percentage of female committee members—are present. However, these statistical measures miss other very distinctive treatments that male chairs used when addressing or acknowledging female witnesses. A woman who testified was usually addressed by her first name by male chairs, whether she was an unknown expert, citizen, familiar lobbyist, or bureaucrat; but a witness who was a man received a title in front of his name, both at the time of introduction and at the conclusion of his remarks. Female chairs used titles with both men and women who were unknown to them and reserved first names for witnesses of both sexes with whom they were familiar.[12] The most egregious example of this sexist treatment occurred in a hearing on a health issue, in which several doctors testified. Although the woman witness clearly stated her title and name as "Dr. Elisa Jones,"[13] the male chair addressed her repeatedly as "Elisa" and finally thanked "Mrs. Jones" for her testimony. Needless to say, perhaps, none of the male doctors were referred to as anything but "Dr. ___."

This all-too-familiar unconscious downgrading of women's authority and position in society, while not affecting the participation rate of female "expert" witnesses, may be providing another view of the sexist culture in committee hearings that permeates the participation rates for some female witnesses and most female committee members. Women experts may be given equal opportunity to speak, but one has to wonder if women's expert testimony is seen as equally credible with men's.

Chairs

In the first analysis of positional roles, we saw that men and women chairing hearings had different speaking behaviors, with female chairs participating less—and interrupting less—than male chairs. In the analysis of witnesses and committee members' speaking behavior, we saw that under a male chair, the chair and his male colleagues on the committee questioned female sponsors early in the hearing. A slightly different approach to examining chair behavior can illuminate what these differences mean to committee members and witnesses. Since chairs play a central role in directing committee discussion, the *type of turns* taken by chairs were coded into one of three categories: *Parliamentary turns* were procedural turns, as when the chair calls a witness or recognizes a committee member who wanted to speak; *clarifying turns* request an affirmation from a previous speaker about the content or meaning of a statement; *question/opinion turns* interject a new idea into the discussion.[14] Beyond taking the floor away from speakers, male chairs also influenced and controlled committee hearing discussions through engaging in substantive comments more than female chairs did. Table 5 examines the sex differences by type of turn.

TABLE 5

Type of Chair Turn Taken by Sex

TYPE OF TURN	MEAN PERCENT MALE	MEAN PERCENT FEMALE	T	P
Procedural	78.6	94.4	−3.01	.009
Clarification	4.7	2.2	1.15	.269
Question/opinion	16.7	3.4	3.32	.007
Total %	100.0	100.0	—	—

Note: The percentages shown are the mean percentages for the chairs of each gender, based on 8 male chairs (who collectively took 391 turns) and 9 female chairs (who collectively took 338 turns).

Men and women chairs did not differ in their use of clarification questions. However, women were more likely to act as facilitators of the hearing, as evidenced through their almost exclusive use of procedural turns. And while men, too, used mostly procedural turns, in one out of six turns (on average), men interjected personal opinions or guided the committee members and witnesses to a topic of their interest. Men used their position of power to control hearings in ways that we commonly associate with the notion of positional power and leadership (see Jones 1989). Indeed, the earlier gendered patterns among committee members may be the interaction of women chairs acknowledging all voices (to be more inclusive) in conjunction with male committee members' more assertive verbal behavior (as evidenced in their interruption and speaking patterns) when there is a woman sponsor, a feminized hearing, or a family issue being considered. In other words, while the conversational terms of the game had been changed through different chair styles, the more dominant verbal behavior of men as committee members became accentuated in a hearing where the chair used a more democratic, less autocratic style (Eagly and Johnson 1990).

DISCUSSION

Although it is commonly believed that women's increased presence in state legislatures will lead to their greater power and influence in policymaking, this assertion has never been tested through observation. As an initial step toward empirically documenting these changes in women's political access to power, I examined their speaking behavior in legislative committee hearings. The findings reported here suggest that women legislators, despite their numerical and positional gains, may be seriously disadvantaged in committee hearings and unable to participate equally in legislative committee hearings. These findings are not actually surprising given our culture and the social construction of male power. Perhaps most disturbing are the results that substantiate Yoder's (1991) thesis of intrusiveness. Contrary to some scholarly expectations and legislators' self-reported behavior (Thomas and Welch 1991), the more women on a committee, the more silenced women became. It may be that women are particularly active in behind-the-scenes negotiations, but informal participation that alters a bill has to surface as formal participation in the form of amendments in committee or on the floor (Hall 1987).

Indeed, in the present research, there was a significant positive relationship between speaking and offering nontechnical amendments ($t = 2.66, p = .02$), with men dominating both. Participation and influence appear to be strongly related, but it may be premature to conclude that women's impact in policymaking is being undermined thoroughly. For example, in the current data set, the bills sponsored by men or women were equally likely to pass out of the first committee hearing (female $n = 4$, male $n = 4$) and become law (female $n = 2$, male $n = 3$). Even among all 360 bills introduced in the 1989 session of the Colorado House there were no statistically significant sex differences in traditional legislative outcome measures such as the final passage of the bill (female 52%, male 55%). However, traditional outcome measures such as passage rates are problematic, because they only capture final points in a complex policymaking process whose participants are neither politically, institutionally, nor socially equal. In attempting to assess women's impact (as chairs, sponsors, or committee members), such acontextual nonsubstantive measures can be seriously misleading. For example, other more sensitive process measures found that women-sponsored bills were significantly more likely to be assigned to more than one House committee (39%, as compared to 27%), were subjected to longer discussions in first committee hearings (averaging 67 minutes, as compared to 54 minutes), and received twice as much hostile witness testimony (12%, as opposed to 6%) (Kathlene, Clarke, and Fox 1991). Thus despite the appearance of equal treatment via the measure of equal success, the more contextual measures suggest that women were even more successful than men since women's bills had a more difficult passage through the legislature. Similarly, the current research demonstrating highly gendered conversational dynamics defies the sex balance in passage rates, indicating that women are able to get their legislation through the process even in the face of men's more vocal participation in committees.

How do we understand women's apparent passage-rate success in the context of such a process imbalance? Without a discourse analysis of the committee discussions, we can only speculate. But some of the gendered patterns may be tapping into differing legislative styles, including men and women's attitudinal approaches toward policy problems and solutions and leadership styles (Eagly and Johnson 1990; Kathlene 1991). However, it would be a mistake to interpret the social and institutional gendered patterns as merely reflections of individual gender differences with no cumulative effect on power and influence. Each gendered layer interacts and reinforces other layers in ways that can enhance existing power (Ragins and Sundstrom 1989), which may indeed thwart women's impact on policymaking. For if legislative bodies rely on committees to inform and guide policy decisions and these same committees are actually the locus of biased information (Jones, Baumgartner, and Talbert 1993), then when men control the policy debate early in the process their gender power will be enhanced within an institutional process that is already structured to "hear" and promote a myopic view of an issue. This has significance for all public policy but I would argue that it is particularly disconcerting for policy issues in which women have a special stake (e.g., women's rights, women's health concerns, and domestic violence). If male chairs have a propensity to interrupt speakers and choose the points to be discussed, women's issues will not receive a fair or (probably) informed hearing. If rank-and-file men are propelled to "take up" and "take away" the floor from rank-and-file women when a female sponsors a bill or the issue heavily affects women, then women's imprint on the policy process will be undermined. The alternative—looking to women chairs as potential power brokers of women's interests—assumes either that their leadership style is similar to men's or that their "different" leadership style will have the same effect as men's on controlling debate in hearings. But there may be little reason, theoretically, to believe that a more democratic or inclusive style that women tend to use will produce the same control over the agenda as the more commonly used autocratic style of men.

Ultimately, to understand gender power differentials, we need to examine how the content of a bill changes as it passes through the process and look closely at who is influencing the changes. Is male verbal aggression directed at women-sponsored bills or women's issues resulting in a transformation of their bills that is disproportionately influenced by male voices? Or do women as chairs or committee members have ways (other than verbal participation in committees) to imprint their concerns on legislation as it moves through the process? For example, whether women's ideas raised in committee hearings are reflected in amendments sponsored by men become an important indirect measure to consider in future analyses.

Examining the speaking behaviors of legislators during committee hearings provides us with yet another view of how powerful gender is in our culture. Bringing more women into politics will not translate directly into a proportionate amount of female power and influence. In fact, there may be a powerful backlash present when women exceed a certain critical mass in a highly masculinized institution such as legislative politics (Yoder 1991). Yet it is clear that the presence of more women legislators will lead to more women sponsoring bills and chairing committees. When women chaired committees the dynamics of the hearing changed for witnesses and female sponsors in complex ways, some of which may provide for more democratic policymaking, particularly if the negative effects of increased verbal aggressiveness by men can be countered. This examination of speaking behavior only begins to tap into gender differences that may have profound substantive implications for legislative decision making.

APPENDIX

The Pearson correlations for the dependent variables in each analysis are presented in Table A-1.

TABLE A-1

Pearson Correlations among Dependent Variables

SPEAKERS	% TIME ELAPSED	WORDS SPOKEN	TURNS TAKEN	INTERRUPTS MADE (%)
All speakers[a]				
Words spoken	−.42	—	—	—
Turns taken	−.40	.37	—	—
Interruptions made (%)	−.43	.49	.75	—
Interruptions received (%)	−.31	.29	.61	.42
Committee members only[b]				
Words spoken	−.36	—	—	—
Turn taken	−.45	.82	—	—
Interruptions made (%)	−.11	.21	.42	—
Interruptions received (%)	−.37	.65	.59	−.17
Witnesses only[c]				
Words spoken	−.40	—	—	—
Turns taken	−.24	.83	—	—
Interruptions made (%)	−.21	.31	.39	—
Interruptions received (%)	−.03	.28	.48	.34

[a] Text Table 2, N = 204.
[b] Text Table 3, N = 106.
[c] Text Table 4, N = 69.

Notes

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1. Research on women in management supports these findings in the legislative setting (Rosener 1990).

2. The Colorado House is bound by its own chamber rules, which set procedures for amending bills in committee and on the floor and set speaking time and turn limits during the third reading on the floor (NCSL 1992).

3. Of the 164 House bills that did not become law, 52.4% died in first committee, 17.6% died in second committee (usually this was House Appropriations), 10.3% died on the House floor, 17.6% died in the Senate, and 2.0% were vetoed by the governor.

4. The larger sample of 68 bills were selected from all 360 bills introduced in the 1989 session of the Colorado State House based on a number of criteria: (1) sex of the sponsor, (2) sponsor's terms in the legislature, (3) issue area of the bill, (4) state fiscal impact, and (5) public visibility. The sample of 68 bills was representative of the 360 bills introduced in the 1989 session in terms of these criteria and legislative outcome factors, including House committee hearing outcomes, amendment action, House or Senate floor action, and governor action (see Kathlene 1991).

5. This time elapsed to speaking does not include committee members who were silent during the entire hearing. The measures of words spoken and turns taken captures silent members, where silent speakers' words and turns are coded as 0.

6. I found that sponsor amendments were likely to result

from one of three conditions: (1) the sponsor did not have all the relevant detailed information before the legislative deadline for introducing bills, (2) the amount of the fiscal note had not been determined by the legislative staff prior to bill introduction (typical for the majority of bills), and (3) negotiations with interested groups had already occurred between the time the bill was introduced and its first committee hearing.

7. A high number of turns taken could also reflect interruptions, in which case turns taken would not be a good measure of active speaking involvement. Overall, however, interruptions accounted for only an average of 11.2% of the speaker turnovers. However, this low interruption rate may vary by groups, so that each analysis also included a test of differences in mean words spoken per turn by dividing the number of each speaker's words spoken by the number of full turns the speaker took. A full turn was a substantive utterance and did not include turns restricted to only social introductions or parliamentary procedures. An interrupted turn was likely to be substantive as speakers tended to interrupt during more heated debates of the issue. In each subanalysis reported here, there were no significant differences found in the measure of words spoken per turn. Therefore, I believe that the number of turns taken does accurately measure the extent of speaking engagement.

8. All the committee member regression analyses were run twice—first using freshman status as the tenure variable and then using terms in the legislature (1–4) as the tenure variable. The analyses did not differ based on the measure of tenure used.

9. The variable capturing the feminization of the committee hearing was coded in two different ways: as the three-interval categories described in the text and as a continuous percentage measure. Both variables performed similarly in all the analyses.

10. It was common for the chair to invite the sponsor to express a preference for the order of the witnesses. Sometimes the sponsor was interested in ordering all of the witnesses, but usually the sponsor only had a preference for who spoke first. Therefore, any gender differences found in the amount of hearing time elapsed before speaking could be an indicator of gendered *sponsor* treatment of the witnesses as much as, if not more than, differential treatment by the chair.

11. A full interaction regression model was also run but the sex/hearing interaction terms did not significantly improve the explained variance. The only difference between the main-effects hearing characteristics and the interaction model was the emergence of male interruption patterns that accounted for the increased interruptions made under a female chair and at a family hearing.

12. It is not hard to distinguish between familiar and unfamiliar witnesses. When a chair knows the witness, he or she usually make a remark indicating their familiarity, such as "Nice to see you again" or "It's been a long time since you've come to this committee." A witness who is unknown elicits slower and more careful (if not mistaken) enunciation of their name, along with the chair offering precise directions to the witness: "State your name and whom you represent, for the tape."

13. Dr. Elisa Jones is a fictitious name.

14. The categories of the type of turns resembles Hall's (1987) coding of chair's turns as *administrative* vs. *substantive*, although I further breakdown substantive remarks based on my personal observations, in committee hearings, that chairs become involved in the issues of the hearing in two distinct ways.

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DOMESTIC POLITICAL AUDIENCES AND THE ESCALATION OF INTERNATIONAL DISPUTES

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International crises are modeled as a political "war of attrition" in which state leaders choose at each moment whether to attack, back down, or escalate. A leader who backs down suffers audience costs *that increase as the public confrontation proceeds. Equilibrium analysis shows how audience costs enable leaders to learn an adversary's true preferences concerning settlement versus war and thus whether and when attack is rational. The model also generates strong comparative statics results, mainly on the question of which side is most likely to back down. Publicly observable measures of relative military capabilities and relative interests prove to have no direct effect once a crisis begins. Instead, relative audience costs matter: the side with a stronger domestic audience (e.g., a democracy) is always less likely to back down than the side less able to generate audience costs (a nondemocracy). More broadly, the analysis suggests that democracies should be able to signal their intentions to other states more credibly and clearly than authoritarian states can, perhaps ameliorating the security dilemma between democratic states.*

An international crisis occurs when one state resists a threat or demand made by another, with both taking actions that suggest that the dispute might be decided militarily. Crises are frequently characterized as "wars of nerves." Measures such as troop deployments and public threats make crises *public* events in which domestic audiences observe and assess the performance of the leadership. For reasons linked to this public aspect of crises, state leaders often worry about the danger that they or their adversary might become locked into their position and so be unable to back down, make concessions, or otherwise avoid armed conflict.

In this article I model an international crisis as a political "war of attrition." The formalization is motivated by an empirical claim, namely that crises are public events carried out in front of domestic political audiences and that this fact is crucial to understanding why they occur and how they unfold. I characterize crises as political attrition contests with two defining features. First, at each moment a state can choose to attack, back down, or escalate the crisis further. Second, if a state backs down, its leaders suffer *audience costs* that increase as the crisis escalates. These costs arise from the action of domestic audiences concerned with whether the leadership is successful or unsuccessful at foreign policy (Fearon 1990, 1992; Martin 1993).

The formalization has three major benefits. First, it helps answer an important question about the origins of war that is missed in the informal literature and begged by existing formal models of crisis bargaining. Briefly, if fighting entails any cost or risk, then rational leaders would not choose war until they had concluded that attack was justified by a sufficiently low chance of an acceptable diplomatic settlement. Thus another way to ask the question "Why do wars occur?" is to ask what leads states to abandon the hope of a cheaper, nonmilitary resolution. A theoretical answer requires us to explain how a state with rational leaders would *learn*. During a crisis, how do leaders come to revise their beliefs about an opponent so that attack is preferred to holding out for concessions? I shall argue that neither the informal nor the formal literature on international conflict supplies satisfactory answers.

The answer suggested here is that audience costs are an important factor enabling states to learn about an opponent's willingness to use force in a dispute. At a price, audience costs make escalation in a crisis an informative although noisy signal of a state's true intentions. They do so in part by creating the possibility that leaders on one or both sides will become locked into their position and so will be unable to back down due to unfavorable domestic political consequences. I find that in the model, a crisis always has a unique *horizon*—a level of escalation after which neither side will back down because both are certainly locked in, making war inevitable. Before the horizon is reached, the fear of facing an opponent who may become committed to war puts pressure on states to settle. The model thus captures a common informal story about international crises—that their danger and tension arise from the risk of positions hardening to the point that both sides prefer a fight to any negotiated settlement.

The second major benefit of the formal analysis is a set of comparative statics results that provide insights into the dynamics of international disputes. The strongest and most striking of these bear on the question of which state is more likely to concede in a confrontation. I find that regardless of the initial conditions, the state more sensitive to audience costs is always less likely to back down in disputes that become public contests. The intuition is that the greater the domestic cost for escalating and then backing down, the more informative is the signal of escalation and the less escalation is required to convey intentions. A stronger domestic audience thus allows a state to signal its true preferences concerning negotiated versus military settlements more credibly and more clearly.

This result and the audience cost mechanism underlying it suggest hypotheses about how state structure might influence crisis bargaining. For example, if actions such as mobilizing troops create larger audience costs for democratic than for authoritarian leaders, then democratic states should be less inclined to bluff or to try "limited probes" in foreign policy—to make military threats and then back off if resistance is met. More broadly, stronger domestic audiences may make democracies better able to signal intentions and credibly to commit to courses of action in foreign policy than nondemocracies, features than might help ameliorate "the security dilemma" (Herz 1950; Jervis 1978) between democratic states.

The comparative statics results also speak to the question of how relative military capabilities and relative interests influence the outcomes of international disputes. Conventional wisdom suggests that the state with inferior military capabilities, or with fewer "intrinsic interests" at stake, is more likely to back down (e.g., George and Smoke 1974, 556–61; Jervis 1971; Snyder and Diesing 1977, 189–95). Surprisingly, in the model, neither the balance of forces nor the balance of interests has any direct effect on the probability that one side rather than the other will back down once both sides have escalated. The reason is that in choosing initially whether to threaten or to resist a threat, rational leaders will take into account observable indices of relative power and interest in a way that tends to neutralize their impact if a crisis ensues. For example, a militarily weak state will choose to resist the demands of a stronger one only if it happens to be quite resolved on the issues in dispute and so is relatively willing to escalate despite its military inferiority. The argument implies that observable aspects of capabilities and interests should strongly influence who gets what in international politics but that their impact should be seen more in uncontested positions and faits accomplis than in crises. Which side backs down in a crisis should be determined by relative audience costs and by *unobservable*, privately known elements of states' capabilities and resolve.

The third major benefit of the analysis is slightly more technical. The model clarifies how international crises differ structurally from the classical war of attrition studied by economists and theoretical biologists (Maynard Smith 1982, chap. 3; Tirole 1989, chap. 8). In the classical case, two firms (or animals) compete for control of a market (or territory) that is not large enough to support both at a profit. The competition lasts until one or both players "quit." International crises are analyzed here as a war of attrition that differs from the classical model in two important respects. First, in crises state leaders possess an additional option beyond continuing the contest or quitting—*they can always choose to attack*. Second, whereas in the classical war of attrition both sides pay costs for continuing the contest, in international crises it is empirically more plausible to assume that only the side that backs down suffers audience costs.[1] The existence of a military "outside option" along with audience costs proves to have major consequences for strategic behavior. Together they create the possibility of "lock in" and thus give crises a horizon. More technically, whereas the classical war of attrition has an infinity of (asymmetric) equilibria involving delay, the game studied here has a unique equilibrium distribution on outcomes up to the horizon.

First, I briefly review the relevant formal literature and also elaborate the theoretical puzzle: Given incentives to misrepresent, how can states involved in a dispute rationally reach the conclusion that the opponent would prefer war to backing down? I then informally discuss possible answers, arguing for the centrality of domestic audience costs, model an international crisis as a political attrition contest to examine the logic of equilibrium behavior, and, finally, draw some general conclusions.

THE THEORETICAL PUZZLE

The costs and risks of war supply states with strong incentives to locate nonmilitary settlements that both sides would prefer to a fight. Most often, it seems, their efforts are successful: *very* few international disagreements become wars. This may seem unsurprising at first glance. One might expect that given the incentives to avoid war, state leaders who disagree on some issue could simply tell each other what they would be willing to accept rather than fight, and then choose a mutually acceptable bargain. The problem, however, is that states can also have strong incentives to misrepresent their willingness to fight in order to gain a better deal. Given these incentives, quiet diplomatic exchanges may be rendered uninformative about a state's preferences. For example, in the Cuban Missile Crisis Kennedy did not ask Khrushchev what he would do if the United States were to impose a blockade or to attack the missile sites in Cuba: answers would have been almost worthless as indicators, due to Khrushchev's incentives to misrepresent (and Kennedy may also have had an incentive not to tip his hand) (cf. Wagner 1989, 197).

States in a dispute thus face a dilemma. They have strong incentives to learn whether there are agreements both would prefer to the use of force, but their incentives to misrepresent mean that normal forms of diplomatic communication may be worthless. I argue that international crises are a response to this dilemma. States resort to the risky and provocative actions that characterize crises (i.e., mobilization and deployment of troops and public warnings or threats about the use of force) because less-public diplomacy may not allow them credibly to reveal their own preferences concerning international interests or to learn those of other states.

To support this claim it must be shown how such actions can credibly reveal that a state would prefer using force to making concessions. In particular, how is it that actions like mobilization and public warnings allow learning? If states can have incentives to misrepresent their willingness to use force, why should such actions be taken as credible indicators?

For the most part, the informal literature on international conflict and the causes of war takes it as unproblematic that actions such as mobilization "demonstrate resolve." The literature has focused instead on how psychological biases may impair a leader's ability to interpret crisis signals (e.g., Lebow 1981; Snyder and Diesing 1977, chap. 4; Jervis, Lebow, and Stein 1985). The prior question of how a rationally led state would learn in a crisis, given incentives to misrepresent, has not been answered in a theoretically thorough or satisfactory way.

Consider the inference problem faced by a state whose adversary in a dispute has just mobilized troops. If rational, the state's leaders should increase their belief that the adversary will fight only if a high-resolve adversary is more likely to mobilize than an adversary that in fact prefers backing down to war. Thus, if mobilization is to convey information and allow learning, it must carry with it some cost or disincentive that affects low-resolve more than high-resolve states. In Spence's (1973) terms, mobilization (or any other move in a crisis) must be a *costly signal* if it is to warrant revising beliefs. Costless signals, which often include private diplomatic communication and sometimes more public measures, will be so much "cheap talk," since a state with low resolve may have no disincentive to sending them.[2]

To explain how states learn in a crisis, we need to know what makes escalation or delay costly for a low-resolve state that in fact prefers making concessions to military conflict. It is tempting to answer "the risk of war", but this would beg the question since we are trying to establish how this risk arises in the first place. I shall argue that the role of domestic political audiences has typically been crucial for generating the costs that enable states to learn. First, however, I briefly review how the published formal literature on crisis bargaining has addressed the issue.

A number of studies have developed models in which states rationally update their beliefs about an adversary's resolve in the course of a crisis (Bueno de Mesquita and Lalman 1992; Fearon 1990, 1992; Kilgour 1991; Morrow 1989; Nalebuff 1986; Powell 1990; Wagner 1991). Though this is not always apparent, the mechanism that enables learning in each case is costly signaling.

While updating of beliefs occurs in these models, they actually do not address the question of how and why states might rationally come to conclude that fighting was preferable to holding out for concessions. The reason is that almost all of the models have *finite horizons*: the modeler exogenously determines that one of the states in the game will have a final choice between backing down or fighting. In effect, one player will ultimately have no choice but to "take it or leave it," and this restriction creates a cost for escalation. In actual crises, by contrast, whenever a state has the option of attacking it also has the option of delaying or doing nothing. If there are horizons in actual crises they arise *endogenously*, as a consequence of the fact that for some reason waiting ultimately becomes an undesirable choice. Models that exogenously fix a horizon cannot explain why a state would choose to use force (and thus why wars occur) because they cannot explain what makes force preferable to holding out for concessions by the other side.

There are two partial exceptions to this argument. Nalebuff's (1986) and Powell's (1990) models of nuclear brinkmanship have something like an infinite horizon: they allow states to escalate in a crisis indefinitely, until one side backs down or nuclear war occurs. However, in these models states never *choose* to attack. Instead, war can occur only as a result of an accident beyond either side's control. Thus these formalizations cannot and were never intended to explain why states would *consciously* choose to abandon peace for war.

Historically, war has virtually always followed from the deliberate choices of state leaders, if not always as the result they originally intended (Blainey 1973; Howard 1983). Since this pattern seems likely to continue even in the nuclear age, it makes sense to ask how states could reach the conclusion that attack was worth choosing.

AUDIENCE COSTS, STATE STRUCTURE, AND LEARNING IN INTERNATIONAL DISPUTES

I shall consider several types of costs that could serve to make the actions that states take in crises informative about their actual willingness to fight. I argue that while there are several plausible candidates that may play a role in specific cases, audience costs are probably most important and characteristic of crisis bargaining. I shall then discuss variations in audience costs across regime types, suggesting that they may be most significant in states where foreign policy is conducted by an agent on behalf of a principal, as in democracies.

Signaling Costs in Crises

Two sorts of costs that leaders face for backing down in a crisis should be distinguished. First, there is the domestic and international price for conceding the issues at stake, which is the same regardless of when concessions are made or after how much escalation. Second, there are whatever *additional* costs are generated in the course of the crisis itself. By the costly signaling argument, only such added costs can convey new information about a state's resolve. To ask what enables learning in a crisis—and thus why some states ultimately choose to attack—is to ask what makes escalating and then backing down worse for a leader than simply conceding at the outset.

There are a number of possible mechanisms, grouped here into three categories: physical costs, costs linked to the risk of accidental or preemptive war, and international and domestic audience costs.

The first class includes the financial and organizational costs of mobilizing and deploying troops and also simple impatience ("time preferences") on the part of state leaders. The economic burden of mobilization is sometimes significant enough to convey information about resolve. The fact that Israel's economy cannot bear full mobilization for very long may make Israeli mobilization unusually informative (Shimshoni 1988, 110). One could also argue that the enormous costs of Desert Shield, given well-known U.S. budget constraints, helped make the deployment a (partially) credible indicator of Bush's preferences (Fearon 1992, 153–54). But since the early nineteenth century, the financial costs of mobilization rarely seem the principal concern of leaders in a crisis, particularly in comparison to how their performance looks to domestic and foreign audiences. In few cases do financial costs seem to be what makes crises into *political* "wars of nerves."[3]

For similar reasons, pure time preferences appear less significant a signaling mechanism in crises than in buyer–seller bargaining and other economic examples.[4] Under time preferences, delay in a crisis would be a costly signal because a leader with a high value for settlement versus war is relatively more impatient to enjoy whatever benefits a resolution would allow. If state leaders are sometimes impatient for a deal, it seems more often due to domestic political pressures (e.g., American elections or Gorbachev or Yeltsin's need for cash) than to a pure preference by the leader for "territory today rather than next month."

The second class of signaling costs concerns risks of war that are generated in some direct way by crisis escalation. Schelling's famous notion of the "threat that leaves something to chance" falls into this category (1960, chap. 8). Schelling suggested that in nuclear crises, at least, escalation or delay might create a risk of war resulting from something other than the deliberate choices of state leaders (e.g., a mechanical mishap or an unauthorized launch). If such risks exist, then escalation in a crisis will be a costly signal of resolve, since the risks are less worth running for a state with low interest in the issues at stake.[5]

In the Cuban Missile Crisis, American decision makers did indeed worry about the risk of war stemming from a mechanical or a command-and-control accident (e.g., Blight and Welch 1990, 109, 311). But even in this most intense of all nuclear crises (a "best case" for the threat-that-leaves-something-to-chance argument), the key decision makers were much more concerned about risks of war connected to what the other side would *choose* to do. While the risk of accidental war may contribute to crisis learning, it rarely, if ever, seems to provide the main cost of escalating a dispute.[6]

First-strike advantages, or incentives for preemptive war, provide a more appealing explanation in this class. If escalating a crisis entails a risk the other side will conclude that concessions are unlikely enough to justify seizing a first-strike advantage, then escalation might be a costly signal of resolve. By running a real risk of preemption, delay in a crisis might credibly reveal a high willingness to fight rather than concede.

This mechanism appears to have figured in some historical cases. For example, part of what made the Russian mobilization in 1914 an informative signal of Russia's willingness to fight was that it was undertaken in the knowledge that it would increase Germany's incentive to choose preemptive war (Fearon 1992, chap. 5).[7] In theory, however, first-strike advantages could also have the opposite effect. A state might conclude that since the adversary has not so far availed itself of a first strike, it must be *more* willing to back down than initially anticipated. Further, major concerns about loss of first-strike advantage do not seem common in case-evidence on international crises, and even when such concerns are present, as in 1914, they often compete with worries about the political disadvantages of going first.[8]

While each of the preceding mechanisms may well foster crisis learning in some cases, I would argue that none fits with our intuitive sense of what it is that makes international crises into political wars of nerves. The reason is that none of these mechanisms recognizes the public aspect of crises, the fact that they are carried out in front of political audiences evaluating the skill and performance of the leadership. In prototypical cases (e.g., the standoff leading to the 1991 Gulf War, the Cuban Missile Crisis, and July 1914), a leader who chooses to back down is (or would be) perceived as having suffered a greater "diplomatic humiliation" the more he had escalated the crisis. Conversely, our intuition is that the more a crisis escalates, the greater the perception of diplomatic triumph for a leader who "stands firm" until the other side backs down.

Political audiences need not and do not always have this pattern of perceptions and reactions: they are social conventions that are at times resolved differently. For example, leaders of small states may be *rewarded* for escalating crises with big states and then backing down, where they would be castigated for simply backing down. Standing up to a "bully" may be praised even if one ultimately retreats.[9] Nonetheless, at least since the eighteenth century leaders and publics have typically understood threats and troop deployments to "engage the national honor," thus exposing leaders to risk of criticism or loss of authority if they are judged to have performed poorly by the relevant audiences. Two illustrations follow, both taken from eighteenth-century diplomacy. While a wealth of similar examples are available from the nineteenth and twentieth centuries, these earlier cases suggest that political audiences have mattered in international confrontations for a long time.

The Seven Years War (1757–64) between France and Britain was preceded by several years of "crisis" diplomacy—threats, warnings, and troop mobilizations and deployments (Higonnet 1968; Smoke 1977, chap. 8). In response to French demands on the Ohio

River Valley, the Duke of Newcastle chose in late 1755 to send two regiments to America to impress the French with British resolve. The decision distressed several of Newcastle's colleagues and ambassadors, who seem to have felt that the action engaged the honor of the king and so committed the cabinet to a warlike course, perhaps unnecessarily. One wrote, "It requires great dexterity to conduct [these diplomatic and military moves] in such a manner to maintain the honor of King and Nation" (Higonnet 1968, 79). In a later interview with Rouillé, the French minister of foreign affairs, the British ambassador reported that the minister "complain'd very much of the licentiousness of our Publick papers in exaggerating things beyond measure which only served to irritate and stir up animosity amongst the lower sort of People in both Nations without a just cause" (p. 80). This complaint suggests that even in nondemocratic, eighteenth-century France, a minister could be concerned with what I have called domestic audience costs: it seems that British pamphlets could have the effect of increasing Rouillé's costs for acceding to British demands (as they increased Newcastle's domestic costs for ceding French demands).

About 35 years later, Britain and Spain nearly went to war over an obscure incident involving alleged Spanish insults to British seamen who had landed on Vancouver Island, along with competing claims on the territory (Manning 1904). Once again, both states resorted to troop mobilizations, forceful diplomatic notes, and public threats. There is strong evidence that these moves created significant domestic audience costs for Prime Minister William Pitt: "With an election imminent, the Opposition was ready to make the most of any of the Government's mistakes in negotiating" (Norris 1955, 572). Pitt's vote for navy credits in Parliament and his government's publication of an account of the Vancouver incident led opposition politicians almost to clamor for appropriate satisfaction of British honor and right. Pitt would have faced serious domestic political costs for backing down, much larger than if he had chosen initially to pursue a less public and aggressive line in the dispute.[10]

The notion that troop movements and public demands or threats "engage the national honor"—thus creating audience costs that leaders would pay if they backed down—continues strongly through the nineteenth and twentieth centuries. Such costs can be classified according to whether the audience that imposes them is domestic or international. Relevant domestic audiences have included kings, rival ministers, opposition politicians, Senate committees, politburos, and, since the mid-nineteenth-century, mass publics informed by mass media in many cases. Relevant international audiences include a state's opponent in the crisis and other states not directly involved, such as allies. Here the costs of escalating and then backing down would be felt indirectly through injury to the state's reputation for threatening the use of force only when serious.

Leaders engaged in disputes appear to worry about both international and domestic audiences. Domestic audience costs may be primary, however. Backing down after making a show of force is often most immediately costly for a leader because it gives *domestic* political opponents an opportunity to deplore the *international* loss of credibility, face, or honor.[11] Because governments are far more likely to be deposed or to lose authority due to internal political developments than due to foreign conquest and because opposition groups frequently condition their activities on the international successes and failures of the leaders in power, domestic audiences may provide the strongest incentives for leaders to guard their states' "international" reputations. Audience costs thus figure in a domestic system of incentives that encourages leaders to have a realist concern with their state's "honor" and reputation before international audiences.

Agency Relations and Audience Costs

As noted, audience costs have a strongly conventional aspect; how they are felt and implemented depends on shared perceptions and expectations in a society. Nevertheless, the historical norm seems to have domestic political audiences punishing a leader who concedes after having deployed troops more than one who concedes outright. Why this norm? My theoretical results will suggest a possible explanation, which I anticipate here in order to develop some broader points about how audience costs vary across types of states.

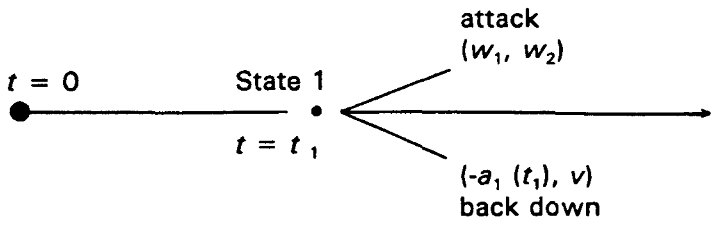
Equilibrium analysis of the crisis model reveals that a state's ex ante expected payoff in a dispute is *increasing* in the degree to which escalatory moves create audience costs for the state's leadership. The reason, in brief, is that greater audience costs improve a state's ability to commit and to signal resolve.

Thus both democratic and nondemocratic leaders should have an incentive to represent that they will pay added domestic political costs for "engaging the national honor" and subsequently backing down. The extent to which such representations are believable, however, depends on the nature of the domestic political institutions that the leadership faces. In democracies, foreign policy is made by an agent on behalf of principals (voters) who have the power to sanction the agent electorally or through the workings of public opinion. By contrast, in authoritarian states the principals often conduct foreign policy themselves. The result here suggests that in the former case, if the principal could design a "wage contract" for the foreign policy agent, the principal would want to commit to punishing the agent for escalating a crisis and then backing down. On the other hand, principals who conduct foreign policy themselves may not be able credibly to commit to self-imposed punishment (such as leaving power) for backing down in a crisis.[12]

Examples of the apparent effect and import of forceful public speeches by democratic and nondemocratic leaders suggest that this argument is at least

FIGURE 1

A Schematic Representation of the Crisis Game



plausible. Repeatedly, leaders in democratic countries have been able credibly to jeopardize their electoral future by making strong public statements during international confrontations. A few prominent examples are Lord Salisbury's speeches during the Fashoda crisis of 1898, Lloyd George's Mansion House speech during the Agadir crisis of 1911, Kennedy's televised speech announcing the presence of Soviet missiles in Cuba, and Bush's many declarations on Kuwait in 1990 (including the "this will not stand" remark).[13] By contrast, even more forceful public bluster by authoritarian leaders (e.g., Hitler, Khrushchev, Mao, and Saddam Hussein) appears to create fewer credible audience costs, and to have correspondingly lower value as signals of intent. For example, it was quite difficult for Western observers to know what to conclude about Saddam Hussein's willingness to fight from his many strong public refusals to pull out of Kuwait in the Fall of 1990. One reason, I would argue, is that it was difficult to know what, if any, added domestic political costs such a tyrant would suffer for making concessions at the last minute.

This is not to suggest that authoritarian states are completely unable to generate audience costs in international confrontations or that democracies can invariably do so. On the one hand, nondemocracies may evolve institutional arrangements in foreign policy that give domestic audiences an ability to sanction decision makers. For example, the politburo after Stalin could sanction the paramount Soviet leader, and eighteenth- and nineteenth-century monarchs could replace unsuccessful ministers. Moreover, since the price of losing power is often greater for a dictator than for an elected leader, a weak or unstable authoritarian regime might be able to create significant *expected* audience costs in a crisis. On the other hand, in democracies the existence of multiple politically relevant audiences may make it difficult for foreign leaders to gauge the costs created by public statements or actions, particularly before elections. The idea that democratic leaders on average have an easier time generating audience costs is advanced here as a plausible working hypothesis that has interesting theoretical and empirical implications.

INTERNATIONAL CRISES AS POLITICAL ATTRITION CONTESTS

I shall describe a model of a crisis as a political attrition contest, informally discuss equilibrium behavior when the states know each other's values for conflict, and then develop the main results on equilibrium with uncertainty about resolve.

Two states—1 and 2—are in dispute over a prize worth $v > 0$. The crisis occurs in continuous time, starting at $t = 0$. For every finite $t \geq 0$ before the crisis ends, each state can choose to attack, quit, or escalate. The crisis ends when one or both states attack or quit. "Escalate" can be thought of either as simply waiting or as taking actions such as mobilizing or preparing troops.

Payoffs are given as follows. If either state attacks before the other quits, both receive their expected utilities for military conflict, w_1 and w_2. These are the states' values for war, incorporating military expectations, values for objects in dispute, and costs for fighting. They can be thought of as levels of "resolve," since the higher w_i, the higher the risk of war state i is willing to run in hope of attaining the prize. Throughout, I shall suppose that neither w_1 nor w_2 is greater than 0.[14]

If state i quits the crisis before the other has quit or attacked, then its opponent j receives the prize while i suffers audience costs equal to $a_i(t)$, a continuous and strictly increasing function of the amount of escalation with $a_i(0) = 0$. I will often consider the linear case $a_i(t) = a_i t$, where $a_i > 0$ is a parameter indicating how rapidly escalation creates audience costs for state i. Figure 1 schematically depicts the structure of the contest, with payoffs indicated in the case that state 1 quits or attacks first at time t_1.[15]

Call this game, with common knowledge of all parameters, G and the subgame beginning at t, $G(t)$. A pure strategy in G is a rule s_i specifying a finite time $t \geq t'$ to quit or attack in every $G(t')$, for all $t' \geq 0$. I shall write $\{t, \text{attack}\}$ for the subgame strategy "escalate up to t, then attack" and $\{t, \text{quit}\}$ for "escalate up to t, then quit."

Equilibrium under Complete Information about Resolve

If the states knew each other's levels of resolve, they could in principle look ahead and anticipate what would happen if they were to escalate and create a crisis. For example, in the linear case where $a_i(t) = a_i t$, they would see that ultimately the audience costs for backing down would be so large that neither would quit: doing so would be strictly worse than attacking. In other words, both states would eventually become "locked in."

However, they would also notice that one side would become locked in *before* the other (excepting certain symmetric cases). Suppose, for example, that audience costs increase linearly and at the same rate on both sides, and that state 1's value for war is higher than state 2's. Then if both sides escalated the crisis, state 1 would reach the point where its leaders preferred conflict to backing down sooner than state 2 would. At this point, state 1 has in effect committed itself not to back down, while state 2 still prefers making concessions to a fight. Thus a rational state 2 would have to back down at this point. Anticipating this at the outset, state 2 would quit immediately rather than pay the larger audience costs that would go with publicly observable escalation.[16]

Thus with complete information about resolve, no public crisis occurs. Instead, if audience costs increase at the same rate on both sides, the state with the lower value for conflict immediately cedes the prize rather than incur costs above those associated with the loss on the issue. If audience costs increase at different rates, then the side with weaker audience effects may be forced to concede even if it has a higher value for war (since it may require more escalation to commit itself to fight).

This equilibrium logic mirrors a logic found in many analytic and diplomatic historical discussions of international disputes. It is often argued that in crises that do not become wars, states look ahead and the side expecting to do worse in military conflict then backs down. But the standard argument does not work by itself: audience costs are required. If it were known that the state with the higher value for war nonetheless preferred making some concessions to a fight, why should the state with the lower value for war necessarily back down? If both prefer concessions to a fight, how can either make a credible threat to go to war and why should this be related to their values for military conflict? Increasing audience costs supply an answer. The state with the higher value for war may be able, in a public crisis, to reach more quickly the point where it prefers conflict to paying the audience costs of backing down.

Incomplete Information about Resolve

With complete information, no public war of nerves occurs because the ultimate outcome can be seen in advance. In a rationalist framework, international crises occur precisely because state leaders *cannot* anticipate the outcome, owing to the fact that adversaries have private information about their willingness to fight over foreign policy interests and the incentive to misrepresent it. I now consider equilibrium in the model in which the states have private information about their willingness to go to war.

Three main theoretical results are developed. Proposition 1 establishes that in any equilibrium in which a crisis may occur, the crisis has a unique *horizon*—an amount of escalation after which neither state will back down and war is certain. Proposition 2 characterizes the set of equilibria in which a crisis may occur. Proposition 3 asserts that in this set, the probability distribution on outcomes is unique up to the horizon time. Throughout, the equilibrium concept is a modification of perfect Bayesian equilibrium (Fudenberg and Tirole 1991) for an infinite game, with an additional restriction ruling out strongly optimistic off-equilibrium-path inferences. Details on the solution concept, along with all proofs, are given in the Appendix.[17]

Preliminaries. Formally, suppose that each state knows its own level of resolve w_i but knows only the distribution of its adversary's resolve w_j. For $i = 1, 2$, let w_i be distributed on the interval $W_i \equiv [\underline{w}_i, 0]$, $\underline{w}_i < 0$, according to a cumulative distribution function F_i that has continuous and strictly positive density f_i.[18] I refer to the crisis game with this information structure and all other elements assumed to be common knowledge as Γ.

In informal terms, f_1 and f_2 represent the states' precrisis beliefs about each other's value for war on the issue in dispute. For example, the more weight f_1 puts on values of w_1 that are close to 0 (as opposed to very negative), the greater is state 2's initial belief that 1 has a relatively high willingness to fight rather than make concessions.

Crises in the Model have a Unique Horizon. This proposition is developed by way of two lemmas, which also help to make clear the logic of strategic choice in the model. I begin with a definition. A crisis has a *horizon* if there is a level of escalation such that neither state is expected to quit after this point is reached. Formally, let $Q_i(t)$ be the probability that state i quits on or before t in some equilibrium. Then $t_h > 0$ is a horizon for Γ if in this equilibrium t_h is the minimum t such that neither $Q_1(t)$ nor $Q_2(t)$ increase for $t > t_h$. If a horizon exists, war has become inevitable by the time it is reached.

Lemma 1 establishes that in any equilibrium in which a crisis may occur, there must exist a horizon. Horizons are thus shown to arise *endogenously*, as a consequence of the equilibrium choices by the states involved. The intuition for this result is straightforward in the case of linearly increasing audience costs: eventually the price of backing down will be so great that even the least-resolved type of state would prefer to attack rather than quit. But the result holds even if "maximum escalation" (arbitrarily large t) will not create large-enough audience costs to commit every type of state to war.

Lemma 1. *In any equilibrium of* Γ *in which both states choose to escalate with positive probability, there must exist a finite horizon* $t_h < \infty$.

Lemma 2 characterizes the behavior of states that choose a strategy that could lead to war. The first part establishes that in equilibrium no state will choose to attack *before* a horizon time. When there is no advantage to striking first, a state unwilling to make concessions will want to delay attack as long as there is any chance that the other side will back down, thus avoiding the risk of an unnecessary military conflict while maximizing the chance of a "foreign policy triumph."

The second part of the lemma shows that a state will choose the strategy of escalating to the horizon and then attacking if and only if its privately known level of resolve, w_i, is sufficiently large. Thus crises in the model *separate* states according to their unobservable willingness to fight over the issues: a highly resolved state credibly reveals its motivation by choosing an unyielding crisis-bargaining strategy. This gives it a greater chance of prevailing if the crisis ends peacefully but at the (unavoidable) price of a greater risk of war (cf. Banks 1990). It follows that for outside observers, crisis outcomes *must* be unpredictable to a significant degree. Comparative resolve strongly influences the outcome, but the fact that resolve is privately known (and unobservable) gives rise to public crises in the first place.

Lemma 2. *In any equilibrium of* Γ *with* t_h *as the horizon and in which escalation may occur, (1) if state* i *chooses* {t, *attack*}, *it must be the case that* $t \geq t_h$; *and (2) state* i *will choose* {t, *attack*} *where* $t \geq t_h$ *if* $w_i > -a_i(t_h)$ *and only if* $w_i \geq -a_i(t_h)$ *(for* $i = 1, 2$*).*

Lemma 2 implies that the ex ante (precrisis) probability that state i will choose a strategy involving attack is $1 - F_i(-a_i(t_h))$, the prior probability, that $w_i \geq -a_i(t_h)$. Thus we can write state i's ex ante expected utility for escalating up to a horizon time t_h and then backing down as $u_i(t_h) = F_j(-a_j(t_h))v + (1 - F_j(-a_j(t_h)))(-a_i(t_h))$.[19] The function $u_i(\cdot)$ proves to play an important role in defining equilibrium strategies and establishing uniqueness. It is easily shown that $u_i(t)$ is continuous and strictly decreasing and that if audience costs increase "enough" with t there is a unique level of escalation $t_i^* > 0$ such that $u_i(t_i^*) = 0$. Loosely, if the opponent j can generate sufficient audience costs by escalating a crisis, there will be a unique level of escalation such that state i would be indifferent between backing down at $t = 0$ and at t_i^*, were this the horizon. I assume in what follows that the states are able to use escalation to generate audience costs at least this large.[20]

Proposition 1, which follows from lemmas 1 and 2 and from the observations about $u_i(\cdot)$, establishes that if a horizon exists it is unique and is defined as $t^* = \min\{t_1^*, t_2^*\}$. To give a bit of intuition, if a crisis were expected to have a horizon longer than t^*, low-resolve states would prefer to quit immediately rather than "bluff" up to t^*, so t^* could not be the true horizon. On the other hand, if the crisis were expected to have a shorter horizon than t^*, then at least one side would have incentives to bluff by escalation that would make equilibrium unsustainable at t^*: the signal sent by escalating would not be informative enough.

Proposition 1. *Let* t_i^* *be the unique solution to* $u_i(t) = 0$ *for* $i = 1, 2$ *and let* $t^* = \min\{t_1^*, t_2^*\}$. *For any equilibrium of* Γ *in which escalation occurs with positive probability, the horizon must be* t^*.

Equilibrium Strategies and Beliefs. Proposition 2 details the incomplete-information crisis game's equilibria that involve escalation. It indicates that there is a family of substantively identical equilibria: all have t^* as the horizon and have essentially identical behavior in the crisis up to t^*. After t^*, the states may choose any time to attack; the payoff structure leaves this open, not incorporating any incentives for either military delay or an immediate strike. (For ease of exposition, I give normal form strategies that can satisfy the perfection requirements detailed in the Appendix.)

Proposition 2. *Take the labels 1 and 2 such that* $t^* = t_2^* \leq t_1^*$. *Let* $k_1 \equiv u_1(t^*) \geq 0$. *The following describes* equilibrium strategies *for state* $i = 1, 2$ *as a function of type,* w_i*:*

For $w_i \geq -a_i(t^*)$, *state* i *plays* {t, *attack*} *with any* $t > t^*$.

For $w_i < -a_i(t^*)$, *state* i *plays* {t, *quit*}, *where* t *is chosen according to any pure strategies that yield the cumulative distributions*

$$\mathcal{Q}_1(t) = \frac{1}{F_1(-a_1(t^*))} \frac{a_2(t)}{v + a_2(t)}$$

for state 1, and

$$\mathcal{Q}_2(t) = \frac{1}{F_2(-a_2(t^*))} \frac{k_1 + a_1(t)}{v + a_1(t)}$$

for state 2, both on the interval $[0, t^*]$.

The states' beliefs in equilibrium *are given as follows.*

For $t \leq t^*$, *state* i *believes that the probability* $j \neq i$ *will not back down (i.e.,* $Pr(w_j \geq -a_j(t^*)|t)$*) is*

$$\frac{v + a_i(t)}{v + a_i(t^*)}.$$

For $t > t^*$, *state* i*'s beliefs follow by Bayes' Rule in accord with the opponent's strategy for attacking. For any* $t > t^*$ *off the equilibrium path, let* i *believe that* $w_j > -a_j(t^*)$ *and is distributed according to* F_j, *truncated at* $-a_j(t^*)$.

Proposition 3. *In any equilibrium of* Γ *in which escalation may occur, the equilibrium distribution on outcomes before the horizon time* t^* *implied by proposition 2 is unique.*

An Informal Description of Equilibrium Behavior. Equilibrium behavior in the incomplete information game has the essential features of a war of nerves. At the outset, one side is expected to prefer to make concessions quietly, without a public contest. This state concedes with some probability (k_1/v) at $t = 0$. If it does not make concessions, then its adversary immediately raises its estimate of the state's willingness to fight, and the war of nerves begins. Neither side knows whether or exactly when the other might be locked in by increasing audience costs, but beliefs that the other prefers war to making concessions steadily increase as audience costs accumulate. The reason is that states with low resolve are increasingly likely to have backed down, the more the crisis escalates. Ultimately, in crises that reach the horizon, the only sorts of states remaining have relatively high values for war on the issue. At this point, both sides prefer conflict to backing down, and both know this: attack thus becomes a rational choice.

At a price, then, audience costs enable the states to learn about each other's true willingness to fight over the interests involved in the dispute.[21] The price is paid in two ways. First, a state may escalate or delay for a time and then quit when its adversary matches it. Though the state is still unsure if the adversary really *would* be willing to fight rather than make concessions, its belief that this is possible has increased and it finds it worthwhile to cut its losses. Second, two states may escalate up to the horizon and then fight, even though one or both would have preferred making immediate concessions rather than this outcome. The dilemma created by private information and incentives to misrepresent is that neither can reliably learn that the other would be willing to go this far without taking actions that have the effect of committing both sides to a military settlement.

One further feature of equilibrium in the model deserves comment before I turn to more specific comparative statics results. The more a crisis escalates, the less likely is either side to back down (regardless of precrisis beliefs). In technical terms, the hazard rate is decreasing: the probability that one's opponent will quit after (say) five escalatory moves is less than the probability that the state will quit after four moves. Thus, as escalation proceeds, states in the model gradually become more pessimistic about the likelihood that the adversary will concede after the next round, and outside observers become increasingly concerned that war may be "inevitable."

AUDIENCE COSTS, CAPABILITIES, AND INTERESTS IN INTERNATIONAL DISPUTES

Comparative statics analysis of the equilibrium yields theoretical insights into how three variables affect state behavior and crisis outcomes. I consider in turn the impact of audience costs, relative military capabilities, and relative interests. For expositional convenience, I discuss the case of linearly increasing audience costs, $a_i(t) = a_i t$.

Audience Costs

A striking feature of the equilibrium behavior just described is that the state less able to generate audience costs (lower a_i) is always more likely to back down in disputes that become public contests. This holds regardless of the value of the prize to either side and regardless of the states' initial beliefs about the other's resolve. Thus if actions such as mobilization generate greater audience costs for democratic than for nondemocratic leaders, we should find the democracies backing down significantly less often in crises with authoritarian states.[22]

By itself, intuition can justify the opposite prediction quite easily. One might think that the side less sensitive to its domestic audience would fear escalation less. Knowing this and fearing large costs of retreat, the side with a stronger domestic audience might then be more inclined to back down. But this argument misses the signaling value of escalation for a state with a powerful domestic audience. While such a state may be reluctant to escalate a dispute into a public confrontation, *if it does choose to do so* this is a relatively informative and credible signal of willingness to fight over the issue. That is, the greater the costs created by escalation for a leader, the more likely the leader is to be willing to go to war conditional on having escalated a dispute. Conversely, escalation by a state that will suffer little domestically for backing down says less about the state's actual willingness to fight.[23]

This dynamic has several further implications. First, the signaling and commitment value of a stronger domestic audience helps a state on average, by making potential opponents more likely to shy away from contests and more likely to back down once in them. In the model, a state's ex ante expected payoff increases with its audience-cost rate a_i. This result provides a rationale for why, ex ante, both democratic and authoritarian leaders would want to be able to generate significant audience costs in international contests.

Second, if democratic leaders tend to face more powerful domestic audiences, they will be significantly more reluctant than authoritarians to initiate "limited probes" in foreign policy. Showing this formally requires that we add structure to the model analyzed here, which does not represent an initial choice of one state to challenge or threaten the other. When such an option is added—say, state 1 chooses whether to accept the status quo or to challenge state 2—it is easily shown that the less sensitive state 1 is to audience costs, the greater the equilibrium probability that the state will try a limited probe.[24]

Third, when large audience costs are generated by escalation, fewer escalatory steps are needed credibly to communicate one's preferences. (Formally, the expected level of escalation decreases with a_1 and a_2.) Thus crises between democracies should see significantly fewer escalatory steps than crises between authoritarian states—an empirically supported prediction (Russett 1993, 21).

Finally, the equilibrium results bear on the question of how regime type influences the risk that a crisis will escalate to war. When two states in the model have the same audience cost rates $a = a_1 = a_2$, the risk of war conditional on a crisis occurring proves to be *independent* of a, other things being equal (cf. Nalebuff 1986).[25] As audience-cost rates diverge, the high-audience-cost state becomes more likely to escalate, while the lower-audience-cost state becomes more likely to back down. The net effect on the risk of war may be positive or negative, although it is positive for a broad range of plausible parameter values. For example, whenever the distribution of w_1 is uniform, the risk of war (given a crisis) *strictly increases* as a_1 increases above a_2.[26] The model thus suggests a theoretical mechanism that could conceivably help explain the observation that crises between democracies and nondemocracies are more war-prone than are crises between democracies (Chan 1984; Russett 1993). In the model, democratic leaders have a structural incentive to pursue more escalatory, committing strategies when they face authoritarians than when they face fellow democrats, and this can generate a greater overall chance of war.

Relative Capabilities and Interests

Two of the most common informal claims about state behavior in international crises are that (1) the militarily weaker state is more likely to back down and (2) the side with fewer "intrinsic interests" at stake is more likely to back down. These arguments are problematic. If relative capabilities or interests can be assessed by leaders prior to a crisis and if they also determine the outcome, then we should not observe crises between rational opponents: if rational, the weaker or observably less interested state should simply concede the issues without offering public, costly resistance. Crises would occur only when the disadvantaged side irrationally forgets its inferiority before challenging or choosing to resist a challenge (Fearon 1992, chap. 2).

A second striking result from the equilibrium analysis is that observable measures of the balance of capabilities and balance of interests should be unrelated to the relative likelihood that one state or the other backs down in crises where both sides choose to escalate.

In formal terms, observable capabilities and interests influence the distribution of the states' values for going to war and thus the states' initial beliefs about each other's willingness to fight (f_1 and f_2). For example, the more the balance of military power favors state 1, the more state 1—and the less state 2—is initially expected to be willing to use force. Regarding interests, the more the issues in dispute are initially thought to be important for, say, state 1, the more state 1 is initially expected to be willing to fight rather than back down.

In equilibrium, the initial distributions of the states' values for war have a direct influence on the probability that one state or the other will concede without creating a crisis. In accord with intuition, the weaker state 2 is militarily or the less its perceived stake, the more likely it is to cede the prize without offering visible resistance.[27] However, if it *does* choose to escalate, then the odds that state 2 rather than state 1 will back down in the ensuing contest,

$$\frac{a_1 v + a_1 a_2 t^*}{a_2 v + a_1 a_2 t^*},$$

are not directly influenced by relative capabilities or interests. For example, when the states have the same audience cost rates ($a_1 = a_2$), they are equally likely to back down in a crisis and equally likely to go to war, regardless of ex ante indices of relative power or interests.

Less formally, the result suggests that rational states will "select themselves" into crises on the basis of observable measures of relative capabilities and interests and will do so in a way that neutralizes any subsequent impact of these measures. Possessing military strength or a manifestly strong foreign policy interest does deter challenges, in the model. But if a challenge occurs nonetheless, the challenger has signaled that it is more strongly resolved than initially expected and so is no more or less likely to back down for the fact that it is militarily weaker or was initially thought less interested.

CONCLUSION

International crises are a response to a dilemma posed by two facts about international politics: (1) state leaders have private information about their willingness to use force rather than compromise, and (2) they can have incentives to misrepresent this information in order to gain a better deal. In consequence, quiet diplomatic exchanges may be insufficient to allow states to learn what concessions an adversary would in truth be willing to make. I have argued that states resolve this dilemma by "going public"—by taking actions such as troop mobilizations and public threats that focus the attention of relevant political audiences and create costs that leaders would suffer if they backed down. Though there are exceptions, the historical norm seems to have domestic audiences punishing or criticizing leaders more for escalating a confrontation and then backing down than for choosing not to escalate at all.

A game-theoretic analysis showed that such audience costs allow states to learn about each other's willingness to fight in a crisis, despite incentives to misrepresent. When escalation creates audience costs for both sides, states revise upward their prior beliefs that the other is willing to use force as the crisis proceeds. If escalation reaches a certain level (the "horizon"), both states prefer fighting to backing down, and both know this. At this point, attack becomes a rational choice.[28]

Equilibrium analysis yielded several novel propositions about how audience costs, relative capabilities, and relative interests influence the outcomes of international confrontations. Some broader implications follow.

A substantial literature in international relations argues that international anarchy, combined with states' uncertainty about each other's motivations, is a powerful cause of international conflict (Glaser 1992; Herz 1950; Jervis 1978; Waltz 1959, 1979). Unsure of each other's intentions, states arm and take actions that may make others less secure, leading them to respond in kind. States' inability to commit themselves to nonaggressive policies under anarchy may exacerbate, or even make possible, such "security dilemmas."[29]

The results of my analysis suggest that domestic political structure may powerfully influence a state's ability to signal its intentions and to make credible commitments regarding foreign policy. If democratic leaders can more credibly jeopardize their tenure before domestic audiences than authoritarian leaders, they will be favored in this regard. For example, in the model examined here, high-audience-cost states require less military escalation in disputes to signal their preferences, and are better able to commit themselves to a course of action in a dispute.

This observation provides a theoretical rationale that might help explain why the quality of international relations between democracies seems to differ from that between other sorts of states. If democracies are better able to communicate their intentions and to make international commitments, then the security dilemma may be somewhat moderated between them. For example, the leaders of a democratic state that is growing in power may be better able to commit themselves not to exploit military advantages that they will have in future, so reducing other states' incentives for preventive attack.[30] Likewise, alliance relations between democracies may be less subject to distrust and suspicion if leaders would pay a domestic cost for reneging on the terms of the alliance, so "violating the national honor" in the eyes of domestic critics.[31]

One tradition within realism argues that democratic leaders are at a disadvantage in the game of realpolitik: domestic constraints reduce their freedom to maneuver and so may prevent them from playing the game as hard or as subtly as it may require (e.g., Morgenthau 1956, 512–26). However, as Schelling (1960) observes, in bargaining a player can benefit from having fewer options and less room to maneuver. I have shown how the presence of a politically significant domestic audience can improve a democratic leader's ability to commit to a course of action *and* to signal privately known preferences and intentions in a clear, credible fashion. These are advantages that could help in the game of realpolitik and might also make democracies better able to cope with the security dilemma.

APPENDIX

A formal statement of the solution concept used for the incomplete information game follows. Because Γ has a continuum of information sets, standard definitions of perfect Bayesian equilibrium (Fudenberg and Tirole 1991) and sequential equilibrium (Kreps and Wilson 1982) do not apply. I propose an adaptation of perfect Bayesian equilibrium, adding a refinement criterion that rules out some optimistic interpretations of out-of-equilibrium play. To avoid measure-theoretic complications, attention is restricted to pure strategy equilibria. Throughout, $i = 1, 2$ and $j \neq i$.

I begin by defining a Bayesian Nash equilibrium for the normal form version of Γ. Here, a pure strategy for state i is a map $s_i: W_i \rightarrow \mathbb{R}^+ \times$ {quit, attack}, where $\mathbb{R}^+$ is the set of nonnegative reals. Using F_i, every s_i induces a unique pair of cumulative distributions $Q_i(t)$ and $A_i(t)$, which are the probabilities that state i quits or attacks by t if i follows s_i. By the properties of cumulative distribution functions, $Q_i(t)$ and $A_i(t)$ are increasing, right-continuous, and have well-defined left-hand limits for all t (Billingsley 1986, 189). Let

$$Q_i^-(t) \equiv \lim_{s \rightarrow t^-} Q_i(s).$$

"State w_i's" expected payoff for $\{t$, quit$\}$, given s_j, is then

$$U_i^q(t, w_i) \equiv Q_j^-(t)v + (Q_j(t) - Q_j^-(t))((v - a_i(t))/2)$$
$$+ A_j(t)w_i + (1 - Q_j(t) - A_j(t))(-a_i(t))$$

or, if $Q_j(t)$ is nonatomic at t,

$$U_i^q(t, w_i) = Q_j(t)v + A_j(t)w_i$$
$$+ (1 - Q_j(t) - A_j(t))(-a_i(t)).$$

Similarly, type w_i's expected payoff for $\{t$, attack$\}$ given s_j is

$$U_i^a(t, w_i) \equiv Q_j^-(t)v + A_j(t)w_i + (1 - Q_j^-(t) - A_j(t))w_i$$
$$= Q_j^-(t)v + (1 - Q_j^-(t))w_i.$$

DEFINITION. $\{t'$, *quit*$\}$ ($\{t'$, *attack*$\}$) *is a* best reply for type w_i given s_j *if*

$$t' \in argmax_t\, U_i^q(t, w_i) \text{ and } U_i^q(t', w_i) \geq max_t\, U_i^a(t, w_i)$$

$$(t' \in argmax_t\, U_i^a(t, w_i) \text{ and } U_i^a(t', w_i) \geq max_t\, U_i^q(t, w_i)).$$

DEFINITION. (s_1, s_2) *is a* Bayesian Nash equilibrium *for the normal form version of* Γ *if (1)* $\{F_i, s_i\} \Rightarrow \{Q_i(t), A_i(t)\}$, *and (2) under* s_i, *every type* w_i *chooses a best reply, given* s_j.

Just as the normal form version of the complete information game G has multiple Nash equilibria, so are there multiple Bayesian Nash equilibria for Γ. However, many of these require states to choose strategies that do not seem optimal or sensible in the dynamic (extensive form) setting. These are ruled out by the "perfection" requirements I shall give.

In the extensive form, a complete pure strategy in Γ is a map s_i: $\mathbb{R}^+ \times W_i \rightarrow \mathbb{R}^+ \times$ {quit, attack}, with the restriction that if $s_i(t', w_i) = \{t, \text{quit}\}$ or $\{t, \text{attack}\}$, then $t' \leq t$. For all $t' \geq 0$, define the "continuation game" $\Gamma(t')$ as follows: (1) payoffs are as in Γ, except beginning at t'; and (2) "initial beliefs" are given by a cumulative distribution function $F_i(\cdot;\, t')$ on W_i. A strategy s_i implies a strategy for state i in every continuation game $\Gamma(t')$; call this $s_i|t'$. Further, using $F_i(\cdot;\, t')$, $s_i|t'$ induces a pair of unique "conditional" cumulative probability distributions $Q_i(t|t')$ and $A_i(t|t')$, analogous to $Q_i(t)$ and $A_i(t)$ already defined. From these, expected payoff functions for $\Gamma(t')$, $U_i^q(t|t', w_i)$ and $U_i^a(t|t', w_i)$ follow as before.

We can now define a weak extensive form solution concept requiring that (1) s_1 and s_2 induce Bayesian Nash equilibria in every continuation game $\Gamma(t)$, and (2) beliefs $F_i(\cdot;\, t)$ are formed whenever possible using Bayes' Rule and s_i, while $F_i(\cdot;\, t)$ can be anything when Bayes' Rule does not apply.

Definition. *$\{(s_1, s_2), F_1(\cdot;\cdot), F_2(\cdot;\cdot)\}$ is a* perfect Bayesian equilibrium *for Γ if*

(A) (s_1, s_2) induces a Bayesian Nash equilibrium in Γ and for all $t \geq 0$, $(s_1|t, s_2|t)$ induces a Bayesian Nash equilibrium in $\Gamma(t)$, using $F_1(\cdot;\, t)$ and $F_2(\cdot;\, t)$; and
(B) for all t such that t is reached with positive probability under s_i (i.e., $Q_i(t) + A_i(t) < 1$), $F_i(\cdot;\, t)$ is $F_i(\cdot)$ updated using Bayes' Rule and s_i.

This solution concept is weak in the sense that it imposes no restrictions on how states would interpret completely unexpected behavior by the adversary. For instance, if state 1 escalates unexpectedly at time t, the concept allows state 2 to conclude that state 1 is without doubt the *least resolved* type $\underline{w}_1$. Further, it would allow state 2 to maintain this belief even as state 1 continued to escalate. Seemingly implausible "optimistic beliefs" of this sort can be used to support a continuum of perfect Bayesian equilibria in Γ for most initial parameter values (with t^* as the maximum possible horizon). The following criterion rules out such optimistic off-equilibrium-path inferences and so refines the set of equilibria.[32] It is stronger than is needed for the proofs that follow, but it has the advantage of a very simple definition:

(C) For all $t > 0$ such that $Q_i(t) + A_i(t) = 1$, $F_i(-a_i(t);\, t) = 0$.

This says that if state i escalates beyond t when it was expected to have quit or attacked prior to time t, then state j believes that i's value for war w_i is at least as great as i's value for backing down at time t. In the text and in what follows, I refer to a pair of strategies (s_1, s_2) and a system of updated beliefs $F_i(\cdot;\, t)$ that satisfy A, B, and C as an *equilibrium* of Γ. I now proceed to proofs of lemmas and propositions in the text, starting with several observations (proofs for observations 2 and 4 are straightforward and are omitted).

Observation 1. *Suppose that in an equilibrium of Γ, $Q_j(t)$ is atomic at t'. Then $\{t', \text{quit}\}$ and $\{t', \text{attack}\}$ are never best replies for state i for any w_i and are chosen with zero probability in equilibrium.*

Proof. Suppose to the contrary that in some equilibrium type w_i chooses $\{t', \text{quit}\}$ where $Q_j(t') > Q_j^-(t')$. State w_i then receives an ex ante expected payoff of

$$Q_j^-(t')v + (Q_j(t') - Q_j^-(t'))((v - a_i(t'))/2) + A_j(t')w_i$$
$$+ (1 - Q_j(t') - A_j(t'))(-a_i(t')).$$

By right continuity of $Q_j(t)$ and $A_j(t)$, the deviation $\{t' + \varepsilon, \text{quit}\}$, $\varepsilon > 0$, yields an expected payoff arbitrarily close to

$$Q_j(t')v + A_j(t')w_i + (1 - Q_j(t') - A_j(t'))(-a_i(t'))$$

as ε approaches 0, which is strictly greater than the payoff for $\{t', \text{quit}\}$. Thus $\{t', \text{quit}\}$ cannot be a best reply for any type w_i. An identical argument applies for $\{t', \text{attack}\}$. Q.E.D.

Observation 1 implies that if in some equilibrium t_h is the horizon, it cannot be that both states choose to quit with positive probability at t_h. Further, we can now write state w_i's equilibrium ex ante expected payoff for $\{t, \text{quit}\}$ as

$$U_i^q(t, w_i) = Q_j(t)v + A_j(t)w_i + (1 - Q_j(t) - A_j(t))(-a_i(t))$$

and state w_i's equilibrium ex ante expected payoff for $\{t, \text{attack}\}$ as $U_i^a(t, w_i) = Q_j(t)v + (1 - Q_j(t))w_i$.

Observation 2. *$U_i^a(t, w_i)$ increases with $Q_j(t)$ for all w_i. Thus in any equilibrium of Γ no type of state i will choose $\{t, \text{attack}\}$ (and $A_i(t) = 0$) whenever there exists a $t' > t$ such that $Q_j(t') > Q_j(t)$.*

Observation 3. *Suppose $t_h > 0$ is a horizon in some equilibrium of Γ in which escalation may occur. Then for all $\varepsilon > 0$ state i quits with positive probability in the interval $[t_h - \varepsilon, t_h)$ for i = 1, 2.*

Proof. If t_h is a horizon, then by definition at least one state (say, i) must quit with positive probability in the interval $[t_h - \varepsilon, t_h]$ for all $\varepsilon > 0$. I first show that this implies that the same must hold for j. If the contrary is true, then in some equilibrium, there must exist a $t' < t_h$ such that for all $t \geq t'$, $Q_j(t) = Q_j(t')$. By observation 2, $A_j(t) = 0$ for $t < t_h$, so $U_i^q(t, \cdot) = Q_j(t)v + (1 - Q_j(t))(-a_i(t))$ for $t < t_h$. $U_i^q(t, \cdot)$ is strictly decreasing in t whenever $Q_j(t)$ is constant and less than 1, so if the contrary is true and $Q_j(t') < 1$, then no type of i would be willing to choose $\{t, \text{quit}\}$ for any $t > t'$, contradicting the hypothesis that t_h is the horizon. If $Q_j(t' < t_h) = 1$, then $\{t < t', \text{quit}\}$ is not a best reply for any w_i, implying that $Q_i(t') = 0$. It follows that t' must equal 0—$\{t, \text{quit}\}$ with $0 < t \leq t'$ never being a best reply for any w_j—so escalation does not occur with positive probability, contradicting the hypothesis. Thus both states must quit with positive probability in the interval $[t_h - \varepsilon, t_h]$ for all $\varepsilon > 0$.

By observation 1, there can be no equilibrium in which both states choose $\{t_h, \text{quit}\}$ with positive probability. Thus in any equilibrium with t_h as the horizon, at least one state (say, i) quits with positive probability in the interval $[t_h - \varepsilon, t_h)$ for all $\varepsilon > 0$. If observation 3 is false, then it must be possible to have an equilibrium in which $Q_j(t)$ is atomic at t_h but j does not quit with positive probability in an interval $[t_h - \delta, t_h)$ for small-enough $\delta > 0$. But then $Q_j(t)$ will be constant and less than 1 for $t \in [t_h - \delta, t_h)$, so by the same logic as in the last paragraph, i will not be willing to choose $\{t, \text{quit}\}$ with $t \in [t_h - \delta, t_h)$, contradicting the hypothesis. Q.E.D

OBSERVATION 4. *Suppose that* $\mathrm{t_h}$ *is the horizon in some equilibrium of* Γ. *By observations* 2 *and* 3, *for* $\mathrm{j} = 1, 2$, $\mathrm{A_j(t)} = 0$ *for* $\mathrm{t} < \mathrm{t_h}$. *Thus* $\{\mathrm{t} > \mathrm{t_h}$, *attack*$\}$ *yields an ex ante expected payoff of* $U_i^a(t, w_i) = Q_j(t_h)v + (1 - Q_j(t_h))w_i$, *while* $\{t < t_h$, *quit*$\}$ *yields* $\mathrm{U_i^q(t, \cdot) = Q_j(t)}v + (1 - \mathrm{Q_j(t))(-a_i(t))}$. *Since* $\mathrm{U_i^q(t, \cdot)}$ *is independent of* $\mathrm{w_i}$, *for all* t *such that* {t, *quit*} *is a best reply for state* $\mathrm{w_i}$, $\mathrm{U_i^q(t, \cdot)}$ *must equal a constant (call it* $\mathrm{k_i}$).

Proof of Lemma 1. Suppose to the contrary that there exists an equilibrium of Γ in which escalation may occur and in which $Q_i(t)$ is strictly increasing for all t for some state i. By observation 2, $A_j(t) = 0$ for all $t \geq 0$ (since for all $t \geq 0$ there exists a $t' > t$ such that $Q_i(t') > Q_i(t)$). And, by observation 4, i's equilibrium expected payoff for $\{t, \text{quit}\}$ is

$$U_i^q(t, \cdot) = Q_j(t)v + (1 - Q_j(t))(-a_i(t)) = k_i,$$

implying that

$$Q_j(t) = \frac{k_i + a_i(t)}{v + a_i(t)} \ (*).$$

However, because $A_j(t) = 0$ for all t, it must be that

$$\lim_{t \to \infty} Q_j(t) = 1.$$

From (*), this will be possible only if

$$\lim_{t \to \infty} a_i(t) = \infty$$

or if $k_i = v$, both of which generate contradictions. If

$$\lim_{t \to \infty} a_i(t) = \infty,$$

then no type of i will be willing to choose $\{t, \text{quit}\}$ for arbitrarily large t, if $\Gamma(t)$ actually occurred. If $k_i = v$, then $Q_j(0) = 1$, implying that j does not escalate with positive probability. Q.E.D

Proof of Lemma 2. Part 1 follows immediately from observations 2 and 3. As for the second part, fix an equilibrium in which escalation may occur and there exists a horizon t_h. Let T_i be the set of times such that for all $t \in T_i$, $Q_i(t)$ is either atomic or strictly increasing. Observations 3 and 4 imply that i's ex ante expected payoff for $\{t \in T_i, \text{quit}\}$ is

$$\begin{aligned} U_i^q(t, \cdot) &= Q_j(t)v + (1 - Q_j(t))(-a_i(t)) \\ &= Q_j^-(t_h)v + (1 - Q_j^-(t_h))(-a_i(t_h)) = k_i. \end{aligned}$$

Also from observation 4, type w_i's ex ante expected payoff for $\{t > t_h, \text{attack}\}$ is

$$U_i^a(t, w_i) = Q_j(t_h)v + (1 - Q_j(t_h))w_i,$$

which is at least as great as $U_i^a(t_h, w_i)$.

There are now two cases to consider. First, if $Q_j^-(t_h) = Q_j(t_h)$, then i's ex ante expected payoff for $\{t < t_h, \text{quit}\}$, $t \in T_i$, is $Q_j(t_h)v + (1 - Q_j(t_h))(-a_i(t_h))$, which implies that i does better to choose $\{t \geq t_h, \text{attack}\}$ if $w_i > -a_i(t_h)$ and only if $w_i \geq -a_i(t_h)$. Second, if $Q_j^-(t_h) < Q_j(t_h)$, then there exists a $\hat{w}_i < -a_i(t_h)$ such that type $\hat{w}_i$ is indifferent (ex ante) between $\{t > t_h, \text{attack}\}$ and $\{t < t_h, \text{quit}\}$ and thus a measurable set of types $W_i' \equiv (\hat{w}_i, -a_i(t_h))$ that strictly prefer $\{t > t_h, \text{attack}\}$ to $\{t < t_h, \text{quit}\}$. But this is impossible. The action $\{t > t_h, \text{attack}\}$ yields $Q_j(t_h)v + (1 - Q_j(t_h))w_i$, while $\{t_h + \varepsilon, \text{quit}\}$ yields $Q_j(t_h)v + A_j(t_h + \varepsilon)w_i + (1 - Q_j(t_h) - A_j(t_h + \varepsilon))(-a_i(t_h + \varepsilon))$. If $A_j(t_h) \neq 1 - Q_j(t_h)$, then for small enough $\varepsilon > 0$, the quit strategy does strictly better for all $w_i \in W_i'$. If $A_j(t_h) = 1 - Q_j(t_h)$, then all $t > t_h$ are off the equilibrium path. Condition C implies that for $t > t_h$, $F_j(-a_j(t); t) = 0$, so $Q_j(t|t_h) = 0$ in all $\Gamma(t)$ for $t > t_h$. But if j will not quit after t_h, then $\{t > t_h, \text{attack}\}$ cannot be a best reply in the continuation games $\Gamma(t > t_h)$ for types in W_i'. Thus $Q_j^-(t_h) < Q_j(t_h)$ is impossible in any equilibrium with $t_h > 0$ as the horizon, and the first case must hold. Q.E.D.

Proof of Proposition 1. From lemma 2, it follows that in any equilibrium with horizon $t_h > 0$, the ex ante probability that state j chooses $\{t \geq t_h, \text{attack}\}$ is $1 - F_j(-a_j(t_h))$. Thus, using observation 3, $U_i^q(t_h - \varepsilon, \cdot)$ can be made arbitrarily close to $F_j(-a_j(t_h))v + (1 - F_j(-a_j(t_h)))(-a_i(t_h))$, which, by consequence, must equal k_i.

Choose labels such that $t_2^* \leq t_1^*$. I show first that t_h cannot be strictly greater than t_2^* in any equilibrium. If it were, then state 2's payoff for $\{t_h - \varepsilon, \text{quit}\}$ would be $k_2 = F_1(-a_1(t_h))v + (1 - F_1(-a_1(t_h)))(-a_2(t_h)) < 0$, which is impossible. Because $A_1(0) = 0$, state 2 can assure itself at least 0 by the strategy $\{0, \text{quit}\}$, and so state 2 would not be willing to choose $\{t, \text{quit}\}$ for any $t > 0$, contradicting observation 3.

Nor can t_h be strictly less than t_2^*. If it were, then both states must expect an equilibrium payoff $k_i = F_j(-a_j(t_h))v + (1 - F_j(-a_j(t_h)))(-a_i(t_h)) > 0$ for $\{t < t_h, \text{quit}\}$. Since for $t \in T_i$, $t < t_h$, $k_i = Q_j(t)v + (1 - Q_j(t))(-a_i(t))$, $k_i > 0$ implies that for both states there must exist a $t_j' \geq 0$ such that $Q_j(t)$ is atomic at t_j' and such that $Q_j(t) = 0$ for all $t < t_j'$. If this were not the case, then for one state, $Q_j(t)$ would require types of j to play $\{t, \text{quit}\}$ when this yielded a payoff of 0 or less, which could not be a best reply for any type. Moreover, it must be the case that $t_1' = t_2'$; if not, then for state i with $t_i' < t_j'$, $\{t_i', \text{quit}\}$ yields a payoff less than or equal to zero. But this contradicts observation 1, since in no equilibrium can both states quit with positive probability at the same time. Thus t_h must equal t_2^* in any equilibrium of Γ and thus, $t^* > 0$ is unique. Q.E.D

Proof of Proposition 2 (Sketch). That the proposed strategies form a Bayesian Nash equilibrium in Γ follows immediately from lemma 2 and the fact that $\mathcal{Q}_j(t)$ is chosen so that all types $w_i < -a_i(t^*)$ are indifferent among $\{t, \text{quit}\}$ for all $t < t^*$ ($k_1 = u_1(t^*)$ and $k_2 = 0$). For the continuation games $\Gamma(t')$, $t' \geq 0$, Bayes' Rule implies that if $\{F_i, s_i\} \Rightarrow \{Q_i(t), A_i(t)\}$, then

$$Q_i(t|t') = \frac{Q_i(t) - Q_i(t')}{1 - Q_i(t') - A_i(t')}$$

and

$$A_i(t|t') = \frac{A_i(t) - A_i(t')}{1 - Q_i(t') - A_i(t')},$$

for $t > t'$ and t' such that $Q_i(t') + A_i(t') < 1$. Notice that when they are defined, $Q_i(t|t')$ and $A_i(t|t')$ are linear transformations of $Q_i(t)$ and $A_i(t)$, respectively. This fact can be used to show that if $\{t, \text{quit}\}$ is a best reply for type w_j given s_i, then it remains so in all continuation games $\Gamma(t' \leq t)$, provided that $Q_i(t') + A_i(t') < 1$ under s_i (and likewise for $\{t', \text{attack}\}$). Thus the strategies given for types given in the proposition form Bayesian Nash equilibria in every continuation game $\Gamma(t')$ up to the earliest time t'' such that for one of the two states, $Q_i(t'') + A_i(t'') = 1$ (if t'' exists). It is straightforward to show that beliefs off the equilibrium path ($t > t''$, if t'' exists) accord with condition C.

Proof of Proposition 3 (Sketch). By proposition 1, the horizon must be t^* in any equilibrium in which escalation may occur. The only question, then, is whether there are equilibrium distributions on outcomes up to t^* that differ from $\mathcal{Q}_1(t)$ and $\mathcal{Q}_2(t)$. Arguments similar to those for observations 1 and 3 establish that any equilibrium quit distributions must be nonatomic and strictly increasing on $[0, t^*)$. But $\mathcal{Q}_1(t)$ and $\mathcal{Q}_2(t)$ are the only nonatomic strictly increasing distributions that make types $w_i < -a_i(t^*)$ willing to quit at any $t \in [0, t^*)$ and also support t^* as the horizon.

Notes

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1. Studying the "diplomacy of insults," Barry O'Neill (n.d.) independently developed an attrition model of international contests that focuses on this same second feature.

2. For the original discussion of costly signaling in economics, see Spence 1973. On cheap talk (which may be informative in some contexts), see Farrell 1988; Crawford and Sobel 1982; Rabin 1990. The crisis signals discussed herein are atypical in that they create costs that are paid only if the signaler takes a certain future action ("backing down") rather than regardless of what the signaler does in the future (as in Spence's classical case). One implication is that these signals can have a commitment (or "bridge-burning") effect. For a discussion of costly signaling in crises, see Fearon 1992, chaps. 3 and 4, and for the seminal treatment of signaling in international relations, see Jervis 1970.

3. For example, the financial costs of sustained mobilization do not appear as a significant factor in the case studies found in Betts 1987; George and Smoke 1974; Lebow 1981; or Snyder and Diesing 1977.

4. Time preferences or opportunity costs are the main rationale given for the costs of delay in models of buyer–seller bargaining. For an overview of these models, see Fudenberg and Tirole 1991, chap. 10. On time preferences in crisis bargaining, see also Morrow 1989, 949.

5. Schelling's own account views the threat that leaves something to chance primarily as a tactical move available to both sides, rather than as a mechanism for revealing private information about resolve (see Fearon 1992, chap. 3; Maxwell 1968).

6. On the importance of distinguishing between "loss of control" in a crisis due to pure accident and "loss of control" due to unanticipated but deliberate decisions, see Powell 1985.

7. Trachtenberg (1991) shows convincingly that the German need to mobilize and attack before Russian mobilization was far advanced was known in both Berlin and St. Petersburg.

8. See the citations in n. 3. I consider how first-strike advantages affect crisis bargaining and escalation in work-in-progress.

9. A number of examples from Balkan conflicts are discussed in Fearon 1992, 184–85.

10. According to Norris, "Pitt was conscious that he must negotiate an agreement acceptable to the new parliament when it met in the autumn, or face political annihilation" (1955, 574). The terms that would have been acceptable to the parliament were (intentionally) made harsher by Pitt's public escalation of the crisis. Spain ultimately backed down and Pitt was much praised for his diplomatic triumph.

11. Significant American examples include the heat Acheson took for "losing China" and Johnson's and Nixon's fears about domestic criticism for sending the wrong signal to the communists over Vietnam) (see, e.g., Gelb and Betts 1979, 220–26.

12. Alternatively, domestic audiences may draw harsh inferences about a leader's competence if the leader backs down in a crisis after escalating. If they do, then this would also create an audience cost that would be felt more strongly in democratic states. On the use of incentive schemes to improve an agent's bargaining power, see Katz 1991.

13. For discussion and citations, see Fearon 1992, chap. 3.

14. Nothing important changes if w_1 and w_2 are allowed to be greater than zero but less than the value of the prize. Also, there is no loss of generality in setting both sides' value for the prize equal to v.

15. Payoffs have been defined except for simultaneous quits or attacks, which do not play much of a role in the sequel. If one state chooses to attack at t and the other chooses to quit or attack at the same time, they receive (w_1, w_2). If both quit at time t, state i receives $(v - a_i(t))/2$.

The assumption that the winner gets v independent of the amount of escalation makes the analysis more tractable without discarding a key feature of crises that distinguishes them from the classical war of attrition, namely, that only one side pays the costs of escalation if a player quits. The assumption that w_i does not depend on t is also made for tractability; it would be both interesting and desirable to relax it. Discounting is omitted for simplicity and so that I can focus on the independent impact of audience costs.

16. For all values of w_1 and w_2, there is one other outcome obtainable in a subgame perfect equilibrium, this one involving the play of weakly dominated strategies: both sides choose $\{0, \text{attack}\}$. If each expects the other to attack immediately, neither has an incentive to deviate. This equilibrium disappears in an alternating-move version of the game. If the two states would be locked in at the same time, then there are three equilibrium outcomes (beyond the weakly dominated one): either one of the two states quits immediately or both play a mixed strategy up to the lock in time.

17. I have omitted proofs of the comparative statics results. These are available on request, along with less-compressed versions of the proofs given here.

18. I assume also that f_1 and f_2 are independent, which is most naturally interpreted to mean that uncertainty is about the opponent's cost–benefit ratio for war rather than about military capability. For a discussion of this issue, see Fearon 1993.

19. This interpretation of $u_i(t_h)$ as i's expected utility for $\{t_h,$ quit$\}$ is valid only if j neither quits nor attacks with positive probability at t_h, but the proofs do not depend on the interpretation.

20. If they cannot (e.g., if $a_1(t) = a_2(t) = 0$ for all t), then there may not exist an equilibrium in which learning occurs.

21. In the linear case, as the audience-cost rates a_1 and a_2 approach zero, the horizon time t^* approaches infinity, meaning that an arbitrarily large amount of delay or escalation is required to credibly signal willingness to fight.

22. In formal terms, the probability that state 1 will back down prior to the horizon time is $a_2t^*/(v + a_2t^*)$. The probability that state 2 will do the same, conditional on the crisis occurring (i.e., lasting longer than $t = 0$), is $a_1t^*/(v + a_1t^*)$. Thus if $a_1 > a_2$, state 2 is more likely to back down than state 1, and vice versa. This result holds for *any* precrisis beliefs, f_1 and f_2.

23. The probability that state i will fight conditional on a crisis occurring is $v/(v + a_jt^*)$, $j \neq i$, and t^* proves to rise as a_i falls, implying the result in the text.

24. In the Cold War period the Soviet Union appeared generally more willing than the United States to threaten the use of military force and then back off or moderate on meeting resistance. This, at any rate, is a reading consistent with standard interpretations of the set of major Cold War crises (e.g., Betts 1987; George and Smoke 1974; and Snyder and Diesing 1977). Certainly the United States has used force on many occasions in Latin America and elsewhere, but military probes to gauge other parties' willingness to resist appear uncommon. Maoz and Russett (1992, 253) report that 62% of 271 post-1945 crises between democracies and "autocracies" were initiated by the autocracy—a number that would extremely unlikely to occur if democracies were just as likely to try military probes.

25. Relevant other things will not be equal if democratic leaders tend to have higher (audience) costs for fighting wars than do nondemocratic regimes. Indeed, the same argument that suggests that democratic leaders will suffer more politically for backing down after escalating a crisis suggests that they will be more sensitive to potential war costs. In the model, crises are less likely to escalate to war the greater are the states' costs for fighting (or, equivalently, the lower v).

26. When the distribution of w_1 is logistic up to $w_1 = 0$, the probability of war given a crisis (Pr(war|crisis)) increases as a_1 increases above a_2 whenever the median value of w_1 is sufficiently low. For example, let $F_1(z) = (1 + \exp(-z - m))^{-1}$ for $z < 0$, and $F_1(0) = 1$. If $v = 1$, then the result holds whenever $m > 1$, which means that the typical state 1 is not willing to run 50% risk of war for the prize. Ultimately, for highly asymmetric situations (very large a_1, very small a_2), Pr(war|crisis) begins to decrease with a_1 in the logistic case. For instance, if $v = 1$ and $m = 5$, Pr(war|crisis) is .045 when $a_1 = a_2$, and reaches a maximum at .11 when a_1 is about 50 times greater than a_2.

27. The probability that state 2 backs down at $t = 0$ is k_1/v. A shift in the balance of power (understood as the probability that 1 would win a war) shifts f_1 to the right and f_2 to the left; this has the consequence of increasing k_1. An increase in the intensity of state 1's interests at stake shifts f_1 to the right (without affecting f_2), which also has the consequence of increasing k_1.

28. An important limitation of the attrition model of crises (a limitation common to most other models of "crisis bargaining") is that it gives states only two ways to resolve a dispute peacefully: one side or the other must "back down." While some evidence suggests that many crises in fact have this aspect (Snyder and Diesing 1977, 248), we would like to know why. A natural next step is to consider models with continuous-offer bargaining (e.g. Fearon 1993; Powell 1993).

29. There is in fact a large set of different arguments lumped under the "security dilemma" heading, but this criticism cannot be pursued here. For a critique of standard "security dilemma" reasoning, see Kydd 1993.

30. Schweller (1992) provides some evidence suggesting that democracies neither engage in nor are the targets of preventive war. Fearon (1993) shows that between rationally led states, preventive war arises from the rising power's inability to commit not to exploit the future bargaining advantage it will have.

31. A similar argument about alliances is developed by Gaubatz (1992), who presents evidence indicating that alliances between democracies last longer than alliances involving nondemocracies. See also Fearon 1992, 355.

32. The refinement is in the spirit of Cho and Kreps (1987) D1 criterion. Even without the refinement, comparative statics results are only marginally weakened for the set of perfect Baysian equilibria of Γ.

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PEACE, WAR, AND THE BALANCE OF POWER

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Every possible relation between the distribution of power and the likelihood of war has been defended somewhere in the literature on international politics: war is least likely if power is distributed equally, war is least likely if power is distributed unequally, and the distribution of power has no effect on the likelihood of war. I try to settle this dispute by examining the effect of expectations about the outcome of war on the choice between war and negotiation. I argue that each of these mutually contradictory propositions can be derived from some plausible set of premises and thus that which one is correct depends on which set of premises best describes a situation. The most important factors affecting the relation between the distribution of power and the likelihood of war are (1) whether the terms of a compromise agreement that might be accepted in lieu of war affect the distribution of power between the antagonists and therefore the probability that the agreement will be enforced and (2) how many states' interests will be affected by the outcome.

The change in the distribution of power caused by the collapse of the Soviet Union has led to renewed interest in the relation between the global distribution of power and the likelihood of war. More recently, there has been controversy about whether increasing the military strength of the Bosnian forces in the Yugoslavian civil war would make a settlement of that conflict more likely. Unfortunately, the literature on international politics is not much help in thinking about questions such as these, since every possible relation between the distribution of power and the likelihood of war has been defended somewhere in it: war is least likely if power is distributed equally (Claude 1962, 56–66); war is least likely if power is distributed unequally (Blainey 1988, 108–24; Organski 1958, 292–93); and the distribution of power has no effect on the likelihood of war (Wittman 1979, 749–51). Moreover, the empirical literature on this subject is equally divided.[1]

Because the arguments that have been offered in support of these contradictory propositions have often been incomplete and intuitive, it seems plausible that the cause of this disagreement has been sloppy reasoning and that the right answer can be found by being more careful about what conclusions follow from what premises. One might therefore be tempted to believe that if one of these propositions can be derived from a formal model it must be true and the others false. I will argue that such an inference would be wrong. I will show how each of these contradictory propositions can be derived from some plausible set of premises and therefore argue that the answer to the question depends on which premises best describe a situation.

Any formal model supporting one or another of these contradictory propositions would be complex. I cannot here fully develop several formal models to illustrate the implications of various alternative sets of assumptions and then compare them. I shall therefore merely sketch several lines of argument that could provide the basis for the construction of formal models, referring the reader to examples in the literature whenever possible. Formal theorists will find this insufficiently rigorous. My excuse is that in this case one must choose between generality and rigor, and some rigorous models in the literature have made unwarranted claims to generality.

First, I shall summarize the reasons that scholars have given in support of each of the various possible relations between the distribution of power and the likelihood of war. I will argue that the sources of their disagreement lie in the degree to which they treat attackers and defenders symmetrically, the assumptions they make about the relation between the balance of forces and the expectations of decision makers, and the assumptions they make about the alternative to war. While they differ in these respects, however, all writers on this subject tend to assume that any conclusions that one might draw about the effects of the distribution of power in a world of two states also hold true in a world of more than two states. Subsequently, I shall consider what justification there is for any of the assumptions these writers have made. I will begin by examining the relation between fighting and negotiating in a world of two states and then examine the effect of adding more states.

The main conclusion is that the most important factor affecting the relation between the distribution of power and the likelihood of war is whether the terms of a compromise agreement that might be accepted in lieu of war affect the relative power of the antagonists and therefore the probability that the agreement will be enforced. If they do, then a second important factor is the number of states whose interests will be affected by the outcome. Thus the answer to the question of what relation one should expect between the distribution of power and the likelihood of war is, "It depends." The point of the analysis is to try to make clear what it depends on.

THE SOURCES OF DISAGREEMENT

The idea that war is least likely if power is distributed equally seems so obvious to many people that it is rare that anyone feels the need to justify it. However, it is clearly based in part on two assumptions: (1) the less likely a state is to be successful, the less likely it is to start a war; and (2) the smaller a state's military forces are relative to its adversary's, the less likely it is to be successful. If one state is known to have no incentive to use force even if it expected to be successful, these assumptions imply that the likelihood of war could be minimized by making that state as powerful as possible relative to its potential adversaries. But since there is no reliable way of knowing which states, if any, will never be tempted to use force, most writers assume that all states are equally likely to contemplate the use of force if the prospects of success are favorable. Many people infer from these assumptions that the probability that either of two potential adversaries will start a war must be least when their forces are equal (see, e.g., Claude 1962, 56; Mearsheimer 1991, 154).[2]

However, if we assume (as is commonly done) that there are only two possible outcomes of war (i.e., one state wins or the other does), then this conclusion does not follow from these premises. Since the distribution of power between two potential combatants affects the probability of success for both and the probabilities of success must sum to one, decreasing one state's probability of success must increase the other's by the same amount. Thus it is not clear why any decrease in the first state's incentive to use force should not increase the other's by the same amount, leaving the probability of war unchanged. If so, the distribution of power would be unrelated to the probability of war.

The simplest (but not the only possible) answer to this objection is that there are three possible outcomes of war, not just two: one or the other side wins, or there is a stalemate. The probability that the war will end in stalemate is greatest when the forces of the two sides are equal. In that case the sum of the probabilities that each state will win is least when their forces are equal. Note, however, that while the probability that either state will use force may, as a result, be least in this situation, both states may still have an incentive to use force that is uncomfortably large.

This reasoning assumes that wars are caused by the states that initiate them, so that one has only to examine the incentives of aggressors to estimate the probability of war. One justification for the claim that inequality of power minimizes the probability of war is based on the fact that it takes at least two states to make a war and assumes that the reasoning just applied to attackers also applies to defenders. If states that are unlikely to be successful are unlikely to use force, then weak defenders are least likely to resist an attack. And if defenders do not resist the demands of aggressors, then wars will not occur (Organski 1958, 292–93). Thus the greater the inequality of power, the less the probability of war. This reasoning implies that if the likelihood of war is to be minimized, not every state can expect to be able to defend its interests.[3]

However, this argument only implies that given an attack, the probability that it will lead to war is less, the greater the superiority of the attacker. But the previous argument implies that the probability of an attack is least, the more nearly equal the two side's forces are. Thus the two together imply that the probability of war is greatest for distributions somewhere between equality and great inequality. To get the conclusion that the probability of war is minimized by inequality, one must show that there is some degree of inequality such that the probability that the victim of an attack will resist is smaller than the probability that a state will choose to attack when their forces are equal. But if defenders must choose between fighting against a superior force and capitulation, they may be willing to fight even if the chances of success are very small. Therefore, the degree of inequality that would be necessary to minimize the probability of war might have to be very great indeed.

However, because war is costly and its outcome uncertain, there ought to be some negotiated settlement that both sides would prefer to the costly lottery associated with war—in which case, the alternative to war will be not capitulation but a negotiated settlement. But then, since the value of any proposed agreement must be compared to the expected value of going to war, the minimum terms that each side will accept will be influenced by how optimistic it is about the outcome of war. Since the balance of power (i.e., the relative size of each side's military forces) influences the expected outcome of war, it influences the minimum terms that each will accept. But if a change in the balance of power makes one state more optimistic about the outcome of war and therefore leads it to increase its demands, it must make the other more pessimistic and therefore willing to accept a less favorable settlement. This is the basis for the claim that a change in the balance of power affects the terms of the settlement that states would accept as an alternative to war but not the probability that war will occur (Fearon 1992; Wittman 1979, 749–51).

But this reasoning assumes that decision makers' expectations about the outcome of war are consistent. If they are not and both sides are optimistic about the outcome, then there may be no negotiated settlement that they both prefer to war (Wittman 1979, 755–57). This provides another justification for the view that war is least likely when power is distributed unequally: Blainey (1988) claims that it is more likely that both antagonists will be optimistic when power is distributed equally than when it is distributed unequally and therefore that states are most likely to be able to agree on a negotiated settlement as an alternative to war when power is distributed unequally. According to Blainey, this explains why wars can be ended by negotiations even though negotiations failed to prevent them.[4] However, Blainey does not make clear why there should be a relation between the distribution of power and the consistency of two states' expectations concerning the outcome of a possible war between them.

These conflicting lines of argument raise two fundamental questions: (1) Is a negotiated settlement always available as an alternative to war? and (2) If a negotiated settlement is available, how does the relative military power of the adversaries affect their choice between war and negotiation? I shall focus on the second question first. To answer it we must examine the relation between fighting and negotiating.

FIGHTING AND NEGOTIATING

Although war and negotiation are usually presented as alternatives to each other, I shall claim that war is best understood as part of a process of negotiation. Thus, while adversaries can certainly choose to negotiate rather than fight, if they fight it is because each sees fighting as a way to influence the outcome of negotiations. The place to begin, therefore, is not with an examination of war but with an examination of bargaining.

Bargaining

The essential feature of bargaining is that two or more actors will be better off if they coordinate their actions than if they do not but that they have conflicting preferences as to which actions are to be selected from some larger feasible set. The paradigmatic example is two people who can divide a sum of money between them if and only if they agree on how it is to be divided. If each of them is interested only in receiving the largest possible amount of money, then what each will be willing to accept will depend on his expectation of what the other is willing to accept. This is the sort of problem that game theory is supposed to help us analyze. It has proved, however, to be extraordinarily resistant to analysis. The purpose of most discussions of bargaining is to defend one or another solution to this problem. However, the question of what agreement rational bargainers will accept is less relevant to understanding why states prefer fighting to negotiating than the question of how long it will take them to accept it, a question that most bargaining theory has little to say about. Thus it may be more relevant to emphasize why what is commonly called "the bargaining problem" is such a hard problem.

Consider, then, two people in a room discussing what agreement they will sign, and suppose one of them demands 90% of the money. This may seem outrageous to the other, but if that is the only agreement the person making this demand is willing to accept, then the only rational choice is for the other to accept it, since 10% of the money is better than nothing. But this reasoning applies to either bargainer. Suppose, then, both demand 90% of the money. Then they will get nothing unless one or the other changes his demand. But which should it be? Suppose after they have been deadlocked for a while one of them considers whether to give in. After all, deadlock could last forever, and it seems reasonable that both bargainers would prefer to accept something now to getting some larger sum later. But precisely because this is true of both, it is as good a reason for either to decide to wait for the other to give in as it is a reason to give in oneself. And this remains true no matter how long they have been deadlocked: if the other is about to give in, it is better to wait for his concession than to give in to him unnecessarily, no matter how much time has already passed.

Consider now the possibility of offering to accept a compromise instead. By conceding some of what the other is demanding, one reduces the potential gain to him of holding out for more: the difference between 90% and 50%, for example, is much less than the difference between 90% and 10%. Perhaps, then, a concession will lead to agreement. However, if the other expects the first concession to be followed quickly by another, then he may reason that while the potential reward to stubbornness has diminished, the probability of getting the reward has increased. Thus a concession may simply make the other more recalcitrant rather than less. But instead of offering further concessions, the bargainer who made the first one may decide that it had the wrong effect and offer no more.

The reason it is difficult to say what individuals should do in an unstructured face-to-face bargaining situation such as this is that every possible division of the money is supported not just by an equilibrium combination of strategies but by a sequentially rational equilibrium. There are two ways of circumventing this indeterminacy: one can extend the definition of rational behavior to include a variety of intuitions as to how a rational person should respond to it, or one can impose some further structure on the situation, (e.g., by requiring the bargainers to communicate offers to each other only in a certain order and at certain fixed intervals).[5] However, any solution that is based on understanding the independent choices of the two bargainers must specify a bargaining strategy for both such that given the behavior specified by the strategy, at the time of the predicted agreement neither bargainer could expect to gain by waiting instead of accepting the prescribed agreement. But since the effect of waiting depends on how the other bargainer responds to it, it must be common knowledge that the prescribed solution is sequentially rational and therefore the value the bargainers place on agreements *now* relative to agreements *later* must also be common knowledge. Thus it is not surprising that two bargainers will often both be optimistic about the effect of holding out for better terms, and it seems entirely possible that even rational bargainers with a good understanding of the problem might take so long to reach an agreement that, had they anticipated the outcome, they both would have preferred to accept the other's offer at the outset.[6]

Bargaining and Fighting

Now suppose that one bargainer grabs the other's arm and twists it painfully behind him while demanding that he give in. Because this has the effect of increasing the cost of disagreement for the victim, standard solutions to the bargaining problem imply that this ought to worsen the terms of the bargain that he ultimately accepts. Note, however, that while this assault may mean that the victim is willing to surrender all the money to the other bargainer, it does not necessarily mean that. While the costs of disagreement are now much greater for the victim than for the attacker, the attacker presumably still (as before) prefers an agreement now to an agreement later and might therefore be willing to agree to less than everything in order to shorten the time to agreement. In other words, this assault may not deprive the victim of all bargaining power. This is because the one who is twisting the other's arm still requires the cooperation of his victim. If not (e.g., if he can simply take the other's money once he is no longer able to resist), then by disabling the other he can take everything.

Perhaps, however, the victim of this maneuver can evade the other's grip and retaliate. If he succeeds in twisting the arm of the first, then the bargaining advantage shifts to him. If not, then a struggle may ensue. At every moment during this struggle there are three possibilities: either bargainer may succeed in pinning the other down, or the struggle may continue. The struggle itself is unpleasant, however, and therefore either bargainer may decide he prefers to offer a compromise in hope that the other will agree and they can quit. Perhaps they might pause in their struggle to consider further offers and counteroffers, or perhaps they might continue to communicate while struggling. Either way, they must, as before, evaluate the terms of any possible offer relative to the possibility of doing better by holding out. However, the value of holding out has now become rather complex. It must include an evaluation of the unpleasantness associated with the struggle, an estimate of how long it might last, an estimate of the probability that either might ultimately emerge as victor, an estimate of the value of each of the possible agreements that might be accepted in the event that either won the struggle, along with the probability associated with each, and an estimate of the range of possible agreements that the other might accept later in the struggle along with the probability associated with each and how long it would take before the other was willing to accept them. Therefore there are now two reasons why bargaining may take so long as to be self-defeating: each can continue to be optimistic that the other will soon tire of the contest and offer a concession, and each can continue to be optimistic that he will soon defeat the other and gain an important bargaining advantage.

Moreover, the use of force adds a qualitatively different element to this situation. Suppose our two bargainers have struggled inconclusively for a while and that one offers a compromise based on his expectation of the consequences of continuing the struggle. If he releases his grip in response to the other's apparent agreement, this may allow the other bargainer to grab him in a hold he would be unable to break and compel him to accept a much worse agreement than the one he offered. Thus, because no one can compel a bargainer not to renege on an offer he has made, once the struggle has begun, neither bargainer may be willing to consider a compromise until its outcome has been decided.

This rather fanciful story about a wrestling match began with one bargainer trying to seize the other in a painful grip in order to force him to agree to a deal that was less favorable to him (and therefore more favorable to the attacker). Before attacking his fellow bargainer, however, he could merely threaten to do it, and the other could then threaten to retaliate in kind. They could then make demands and counterdemands of each other based on those threats. Since no one would have tackled anyone at that point, however, it is possible to doubt that anyone would or that if one did, the other would actually resist. Moreover, even if he did resist and were successful in avoiding being pinned down, he might quickly offer a compromise settlement. Thus each bargainer must choose between accepting some proposed agreement and using force to try to extract a better one. If they both decide to use force, this is not because they have decided not to bargain but because they believe fighting will improve their bargaining position.

Each bargainer's expectations of the consequences of rejecting any particular proposal can be divided into two categories: expectations about the outcome of bargaining in various possible circumstances (including the outright victory of one or the other bargainer) and expectations about the outcome of a fight. How many possible outcomes of a fight are there? One plausible answer is that there are three: one or the other loses, or there is a stalemate. Thus expectations should take the form of a probability distribution over these three possibilities. However, one could argue that no stalemate could last forever—that one or the other would eventually tire and have to submit, though this might take a long time. In that case, one might want to say that there are only two possible outcomes: one wins, or the other does. But if they care about how long it takes for one to win (as surely they would), then we would have to say that there are infinitely many possible outcomes: a victory for one or the other at every possible time at which it might occur.

In thinking of their expectations concerning the outcome of fighting, one must distinguish between how uncertain they are and how consistent their expectations are. They are maximally uncertain if they believe all possible outcomes are equally likely. Their expectations are consistent, however, only if the probability they each assign to every outcome is the same. If they are both maximally uncertain, then their expectations must necessarily be consistent. If not and if each has private information relevant to estimating the probability of the outcome, then their expectations may be inconsistent.

We began by assuming that these two people were bargaining over the division of a sum of money. Before force was introduced into the story, this money would have to be a potential addition to the wealth of each bargainer, so that the cost of failing to agree was simply that they would fail to gain anything. Once force is introduced, however, this is no longer true, since either might be willing to surrender money he already possessed to avoid further physical punishment, and the cost of failing to agree is continuation of this punishment (if bargaining takes place after fighting begins) or the possibility that one may be subjected to this punishment (if bargaining takes place before fighting begins). The distribution that exists prior to bargaining is one possible outcome of bargaining but not the only one, even if an agreement is reached without fighting. Thus whether these people can avoid fighting and whether they can both protect the assets they had at the outset are two different questions.

Suppose now that it is common knowledge that in the event force is used, one would quickly subdue the other (perhaps because he is a muscular athlete while the other is an uncoordinated wimp). Should they then be able to reach an agreement without the need to use force? If so, they must both prefer the expected value of bargaining prior to the use of force to the expected value of first fighting and then bargaining. If they have sufficiently conflicting expectations of the outcome of bargaining after fighting this may not be true, since by resisting when he has no chance of success one bargainer might be able to influence the other's expectation of how stubborn he would be once the struggle was over.[7] Thus their ability to reach agreement without fighting depends on how consistent their expectations are of the outcome of bargaining after the fighting is over.

Now suppose that they appear to be very evenly matched physically but that one has a black belt in karate. If this were commonly known, it would give that person a bargaining advantage. Precisely for this reason, however, if it is not commonly known then the other has good reason not to believe any claims to this effect. Thus, because the factors that are commonly known are evenly balanced, private information may lead to inconsistent estimates of the probability that each will win. But their ability to reach agreement without fighting depends on their having consistent expectations of the outcome of the bargaining that will take place once the fight is over, and this will be impossible if they have inconsistent expectations of the outcome of fighting. Thus, even if they have consistent expectations of the outcome of bargaining in the event that either won, inconsistent estimates of the probability that either will win may imply that there is no agreement prior to the use of force that they both prefer to fighting.

Even without introducing karate into the story, it is possible that because the two are evenly matched they expect that a struggle would be long and arduous, and therefore that each might believe that the other is likely to make a concession long before it is over. Thus, as long as fighting can be interspersed with negotiations, private information about each other's evaluation of the struggle itself can lead both of them to be optimistic about the outcome of fighting, even though it is common knowledge that a struggle fought to a decision would be long and costly for both sides. Therefore anyone who reasons that simply because they were evenly matched physically the prospect of a long and costly struggle would deter them from fighting must implicitly assume that once the fight began it could not be interrupted until one or the other had been defeated.

War and Negotiation

Since wars are fought by collectivities, a war's costs and benefits will fall differently on different individuals, who will therefore have different interests even if they have identical expectations—and they are very unlikely to have identical expectations. Thus wars give rise to intense internal conflicts whose pursuit can influence the course of the war. If we ignore this complication (as the literature on the balance of power does), then I shall claim there is a very close parallel between the story just told and interstate war.

Conventional interstate war is a contest in which each side tries to disarm the other in order to acquire the bargaining advantage to be derived from the unopposed use of force. In the limit, of course, force can be used to destroy one's adversary completely. Usually, however, the winner of a military contest requires the cooperation of its defeated enemy in order to accomplish its aims.[8] Thus "victory" and "defeat" refer to the outcome of the clash between armed forces, not to the political objectives that motivated the conflict.[9]

Military contests may lead not to the defeat of one side or the other, however, but to stalemate. Fighting may last for a long time before it becomes common knowledge that a stalemate exists. But even if stalemate is commonly expected at the outset, the fact that fighting is costly means that it may be possible to derive a bargaining advantage from imposing those costs on the enemy, even though doing so requires subjecting oneself to costs as well. Thus one cannot assume (as many do) that a simple probability distribution over the two outcomes of victory and defeat is an adequate way of representing states' incentives to use force or that a belief that stalemate is the most likely outcome is sufficient to deter its use.[10]

Wars, therefore, are contests in the use of force that are meant to induce one's opponent to agree to something. If the adversaries have inconsistent expectations of the outcome of bargaining after fighting, they are unlikely to be able to reach an agreement without fighting. And if they have inconsistent expectations of the outcome of fighting, they are unlikely to have consistent expectations of the outcome of the bargaining that will follow.

There are a variety of factors that are relevant to estimating the outcome of fighting, including the relative sizes of the opposing forces, the nature of the weapons systems available, the ability of the antagonists to mobilize resources to replace those which are lost in combat, the skill of the opposing forces, the strategies to be employed, and so on. The state of some of these factors (e.g., the size of the opposing forces and the economic base that supports them) is public knowledge. However, information about other factors (e.g., the strategy to be employed) is not public, either because states try to keep them secret or because they have an incentive to mislead their adversaries when they publicize them. The greater the role of factors that decision makers have private information about, the greater the likelihood that their estimates of the outcome of war will be inconsistent. And the more nearly equal the publicly known factors are, the greater the role of factors about which the antagonists have private information. Hence it is most likely that both sides will feel optimistic when the balance of power is approximately equal and that their expectations will be consistent when their power is unequal. This reasoning therefore supports Blainey's claim that conventional war is least likely when power is distributed unequally between the potential combatants.[11]

However, in comparing the choice between war and negotiation with a situation in which two individuals were struggling over the division of a sum of money, I implicitly assumed that agreements were enforceable (even if offers made while struggling were not). I also assumed that there was no relation between wealth and bargaining power. But agreements between states must be self-enforcing, and the correlation between wealth and power is an important feature of international conflict. This means not only that the wealthiest states are likely to be the strongest states but also that states can become stronger by defeating other states and acquiring their resources. Thus, the outcome of one contest can influence the expected outcome of subsequent contests, so that compromise agreements may not be enforceable.

To see why, suppose that two states are considering an agreement to redistribute resources as an alternative to fighting. The range of agreements they both would prefer to fighting depends on their expectations of the outcome of the fight. But if redistributing resources from one to the other worsens the expectations of the first and improves the expectations of the second, then after the redistribution the one who benefited from it may be able to enforce a demand for more. If so, the first agreement is not enforceable and therefore not really available.[12] In that case war can be avoided only if each side prefers the existing distribution to the present value of the distribution that it expects would be agreed to after one or the other was defeated or if one side is sufficiently pessimistic about the outcome of fighting that it prefers to accept defeat in advance.[13] This implies, as we have seen, that war is most likely if the balance is neither equal nor very unequal.

Moreover, states may prefer fighting to negotiating in these circumstances even if their expectations of the outcome of both fighting and negotiating are consistent. Blainey wrote: "When nations prepare to fight one another, they have contradictory expectations of the likely duration and outcome of the war. When those predictions, however, cease to be contradictory, the war is almost certain to end" (1988, 293–94). If compromise agreements are not enforceable, however, states may fight not because their expectations are inconsistent but because they are both sufficiently uncertain of the outcome of war to prefer it to defeat or the status quo. The war ends, therefore, not when the adversaries' expectations become consistent (since they may have been consistent at the outset) but when the ultimate outcome becomes nearly certain.

We have identified, then, two plausible lines of reasoning about the relation between the distribution of power and the likelihood of war. One leads to the conclusion that (other things being equal) there is an inverse linear relationship between inequality of power and the probability of war, since the more unequal the balance of forces, the more consistent adversaries' expectations of the outcome of a military contest will be and therefore (other things being equal) the more likely it is that they will be able to agree on the terms of a settlement as an alternative to war. The other leads to the conclusion that the relation between the distribution of power and the probability of war is nonlinear, since the probability of war is greatest for distributions somewhere between equality and great inequality; however, power has to be very unequal for the probability of war to be lower than it is when it is distributed equally. The crucial difference between these two arguments concerns the enforceability of agreements: the first assumes that all possible compromise agreements are enforceable, and the second assumes that compromise agreements are not enforceable and that therefore adversaries must choose between the status quo and negotiating after one side or the other has been defeated on the battlefield.

Note that both arguments are ceteris paribus arguments, and thus in practice any effect of a change in the distribution of power on the probability of war could be swamped by the influence of other factors. Note also that both assume (implausibly) that any two potential adversaries are equally likely to decide to use force to upset the status quo when faced with the same probability of military success. If instead one state prefers the status quo to any alternative regardless of the distribution of power, both lines of argument imply that the probability of war is least when that state's relative power is maximized.

Both these arguments also assume that only two states are in conflict with each other. However, wars are fought not just by individual states but by coalitions, so that the distribution of power between military antagonists depends not just on the power of individual states but also on which states decide to fight. We must therefore consider how this affects the relation between the distribution of power and the likelihood of war.

ALLIANCES, THE DISTRIBUTION OF POWER, AND WAR

Let us continue to assume, following the previous discussion, that states have resources such as wealth, territory, population, and technology that are assets in fighting wars. Using their knowledge of the assets of potential adversaries, statesmen estimate the likelihood of the outcomes of wars between them. Because the outcomes of wars are also affected by factors that are not common knowledge, their probability estimates may not be consistent. However, given any two potential military antagonists, the more unequal they are with respect to their publicly known assets, the more consistent everyone's expectations will be. But now let us also assume that there are more than two states and that the assets of states can be combined, so that wars can be fought between coalitions as well as individual states.

We have seen that because assets such as wealth, territory, and population lead to military power, gains derived from either the actual or threatened use of force may be cumulative, so that any particular pair of adversaries must consider the effect of some proposed redistribution of these assets on the expected outcome of subsequent conflicts between them. When there are more than two states, this fact also implies that even allies must consider the effect of any redistribution on the expected outcome of subsequent conflicts in which they may be adversaries, and states that do not participate in a conflict must consider the effect of its outcome on some subsequent conflict they might have with one or another of the participants. Controversies about the implications of these facts are the central focus of the literature on the balance of power.

In examining their implications for the relation between the distribution of power and the probability of war, it will be convenient to assume that all the commonly known assets of a state that are relevant to predicting the military outcomes of wars it might engage in can be summarized in a single index represented by a real number, *r* (for "resources"). Using their knowledge of the distribution of these resources, together with any private information they may have, statesmen estimate the likelihood of the outcomes of wars fought between various possible combinations of states. (When states form alliances, the military resources available to the alliance are simply the sum of the resources of the member states.) Let us also assume that wars lead to one of three possible military outcomes: one or the other side's armed forces win, or there is a military stalemate.

Some recent game-theoretic models designed to investigate the stability of international systems have been based on such assumptions. However, these models have also made a very simple and unrealistic assumption about the relation between the distribution of power and the outcomes of wars: if $r_i > r_j$, then it is common knowledge that the probability that *i* will win a war with *j* is 1.0, and winning the war will give *i* control over all of *j's* resources; whereas if $r_i = r_j$, it is common knowledge that the probability of stalemate is 1.0, and a stalemate implies that no resources are redistributed (Niou and Ordeshook 1990; Niou, Ordeshook, and Rose 1989; Wagner 1986). With these assumptions, of course, states may or may not be eliminated (which is the question of system stability) but wars will occur only if negotiation is impossible, since there is neither uncertainty nor private information.

Nonetheless, these models are relevant to our problem, since one of the possible consequences of wars is the elimination of the defeated state or states and the absorption of their resources by the victors and since this fact will affect the incentive of states to participate in wars. Suppose, for example, that two states absorb all the resources of a third. This transfer will not be permanent if, as a result of it, one of the victors is then able to force its former ally to surrender its resources. Consider, then, a system of three states in which the resources are distributed 150, 100, 50. Then the assumptions just stated imply that if the largest state acquires any more resources it will be able subsequently to defeat both the other two. Thus neither of the other states will ally with it, and each will come to the aid of the other if it is attacked by it. Moreover, neither of the smaller states will attack the other, since doing so would allow the largest state to join the attack and gain resources. Therefore no transfers can take place in this situation, and the system is stable.[14]

Suppose, on the other hand, that resources are distributed 100, 100, 100. Then a coalition of any two states can defeat the third, but will it absorb all its resources? Perhaps it will, if the resources of the defeated state are divided equally between them, leading to the distribution (150, 150). In that case, the system is not stable. However, such an agreement would not be enforceable, and nothing prevents either ally from trying to cheat and acquire more. If it does, then the other can only protect itself from this defection by switching to the side of the victim before it is eliminated.[15] And even if this fails to occur, the enemy is defeated, and the distribution (150, 150) is implemented, any exogenous change in either side's resource endowment (perhaps by economic growth) would leave the other unable to avoid defeat. Thus neither of the victorious states would want to eliminate the defeated state entirely. Either or both these possibilities would imply that the victim, while it might lose resources, would not be eliminated, which provides a justification for a "hidden hand" interpretation of the idea of the balance of power: in a system of more than two states, the balance of power will protect the independence of states even if all states seek to expand at the expense of others.[16]

With this conclusion in mind, consider again the distribution (100, 100, 100). In this situation each state is exposed to the possibility that the other two will form a coalition and attack it. On the other hand, any two of them can secure themselves from attack by attacking the third and absorbing its resources until one of them has 150 units, which freezes the system and makes further transfers impossible. Thus there is a strong incentive for expansion even for states that have no interest in acquiring more resources.

This reasoning implies that three-member international systems are stable. But this conclusion implies in turn that five-member systems may not be, for if two states in a five-member system are strong enough to defeat any combination of other states, then they can eliminate two other actors and stabilize their winnings in a three-actor system. The traditional literature on the balance of power focuses on the need for states to ally against a potential hegemon, that is, a state that is on the verge of being able to defeat any combination of other states. In systems with more than three states, however, this reasoning implies that hegemonic coalitions are also a danger.

These conclusions rest on the assumption that the outcome of wars can be anticipated with certainty and that they are common knowledge. It we assume, more plausibly, that knowledge of the distribution of resources between potential adversaries leads to nonzero probability estimates for all the possible outcomes of war and that these probability estimates may not be consistent, then the incentive for coalitions to form in opposition to potentially hegemonic states or coalitions remains, but much else is altered.[17] Uncertainty about outcomes implies that in every military conflict, each side faces some probability of defeat, though this probability can be diminished by increasing the resources on one's own side. In a world of two states, of course, each side can increase the quantity of resources available for military use by reallocating them from other uses; but a state's initial resource endowment (and its capacity for mobilization) implies that there is some upper limit to the quantity of resources it can devote to conflict. Alliances enable states to circumvent this limitation if other countries are willing to cooperate. Uncertainty about what other states will do, however, will lead to uncertainty about the strength of each side, and thus add to the uncertainty that already exists about the outcome of war.

Consider again the distribution (100, 100, 100), for example, and suppose the first two states combine to attack the third. With no uncertainty about the outcome of war, if their attack led to a distribution such as (150, 140, 10) then their gains would be secure, since the largest state could not defeat the other two combined and neither of the other two would allow the other to be attacked without coming to its assistance. With uncertainty, however, the second-biggest state may decide that such a distribution would lead to too great a risk of defeat by the most powerful state. Therefore the second state may want to leave the third stronger than it otherwise would, in order to reduce the threat that its own ally would subsequently pose. However, there is nothing to guarantee that the other states would agree either with its probability estimates or with its attitude toward risk. Suppose that war led to a distribution such as (140, 140, 20). Then, instead of being supported by the third state in an alliance against the first with a comfortable margin of superiority, the second state might find itself faced with a hostile alliance between the first and third states. In order to avoid this outcome, it might therefore agree to allow the first state to make greater gains at the expense of the enemy. This would increase the likelihood of postwar assistance from the third state in any military conflict with the former ally, but at the price of a smaller chance of winning in case such a conflict occurs.

Thus uncertainty implies that states face a dilemma in trying to secure the fruits of military victory. Maximizing security requires maximizing the power of whatever postwar coalition a state expects to belong to, as well as maximizing the incentives of the other members of this expected coalition to participate in it. But with uncertainty these objectives are inconsistent, since the incentive of states to participate in a coalition for the defense of the status quo is inversely related to its power relative to a potentially dominant state. One paradoxical implication of this fact is that while in a two-state world the fact that gains are cumulative implies that compromise agreements are not enforceable, in a world of three or more states the reverse is true: a state threatened by another state can increase the incentive of a third state to help defend it by conceding some of its resources to the threatening state and thereby weakening itself.[18]

What does all this tell us about the relation between the distribution of power and the likelihood of war? In a two-state world one could minimize the likelihood of war by maximizing the power of states that are satisfied with the status quo. However, it is not clear how to identify such states or to guarantee that they would always remain satisfied. Thus if compromise agreements are not enforceable, only equality of power will minimize the probability that either state will choose to use force against the other. In a world of three or more states, however, one must distinguish between the distribution of power among states and the distribution of power between some pair of coalitions that might fight each other. And if settlements are not enforceable, then even dissatisfied states must be concerned about how to protect what they already have. As a result, it is at least possible that threats to upset the status quo would always be met by an overwhelmingly powerful coalition, a possibility that would paradoxically require an equal distribution of power among states.

To see why, bear in mind that (other things being equal) the probability of one side's success in war varies directly with the ratio between its resources and those of its opponent and that the more unequal this ratio the less likely "other things" (i.e., assets that states have private information about) will matter. Thus states that wanted to deter an attack by all possible antagonists would want to maximize the resources available to prevent every other state from upsetting the status quo.[19] Consider again, then, the distribution (100, 100, 100), and assume that no state wanted to diminish the power available to oppose some other state's expansion. Then, even if every state would prefer to have more resources, this distribution would have the same effect as the (150, 100, 50) distribution in the world without uncertainty earlier discussed; that is, no state would want to assist any other in expanding (since even if it expanded as well, the relative power of any coalition subsequently available to oppose the other expansionist state would be diminished), and any state that sought to expand on its own would be opposed by both the other two. Because distributions must be self-enforcing, in other words, even expansionist states would follow a collective security policy.

If this sort of symmetrical policy of deterrence is to succeed, however, any state contemplating expansion would have to be confident that all the resources that might potentially oppose it would in fact be used against it. Consider again the distribution (100, 100, 100) and suppose the first state believes that it has a strategy that will enable it quickly to defeat the second state at an acceptable cost (in spite of the equality of their military resources). If it is to be deterred from trying, it must believe that the third state will oppose it. But will it?[20] This depends not only on the third state's evaluation of the risks of not opposing it relative to the costs of doing so but also on what the third state believes about the second state's preferences; for it would be self-defeating for the third state to protect the second and then find itself facing a coalition between the second state and the first state—that is, in order for all states to follow a policy of collective security, it is necessary not only that they all prefer to follow a policy of maximizing deterrence but that it be common knowledge that they do. But precisely because military resources are distributed so equally, each state has reason to doubt that this is true of the others.[21]

Now that we have seen a connection between the idea of collective security and military deterrence, we can use the literature on deterrence theory to explore this problem further. The deterrence problem is to make one's threats credible when there is reason to doubt one's willingness to carry them out. This problem arises because the person or state making the threat has private information about his willingness to carry out the threat and has an incentive to misrepresent that information to the person or state being threatened. When modeled as a game with incomplete information, the standard conclusion is that to increase the credibility of its threat a state must take some action that it would be less likely to take if it were prepared to carry out its threat than if it were not (Wagner 1992). Doing this is much more difficult in the three-actor system I have described, however, than in the two-actor context in which the problem is normally discussed. For example, each state must try to show that it is prepared to join with another to defend the status quo but not to alter it by supporting another in an attack. It is not clear what actions would serve to discriminate between these two sets of preferences.[22]

Moreover, if one state does attack another, this could be either because it does not share the preferences necessary to support a collective security policy or because it doubts that the others do. Should the others therefore seek to reestablish their credibility by jointly defending the status quo, or should they infer that the attacking state will not cooperate in a collective security system and therefore seek to make it less of a threat by reducing its power? And if they choose the latter alternative, how are they to protect themselves from each other after this is done?[23]

Thus, while an equal distribution of power minimizes the role of private information in estimating the power of the community as a whole relative to the power of a single state that challenges the status quo, private information about the willingness of states to join in the defense of the status quo means that there may nonetheless be inconsistent expectations about whether a challenger would be successful. Consider, on the other hand, a distribution such as (140, 130, 30) and suppose that the two smaller states agree that any further increase in the power of the largest would be unacceptable and declare that they would cooperate to prevent it. If so, then the distribution itself would lend credibility to this claim, and this credibility could be reinforced by the formation of an alliance, the commitment of troops, and all the other actions that states customarily use to signal their intentions to each other. Thus this distribution lessens the role of private information in influencing states' expectations about who will defend whom against what. And the near equality of the two opposing coalitions reduces the incentive of either to use force against the other.

Thus, if wars affect the distribution of power, then (other things being equal) in a world of more than two states the distribution of power that minimizes the likelihood of war is one in which power is distributed sufficiently unequally among states that at least one state (or, if there are four or more states, a coalition of two states) is considered unacceptably close to dominance by the other states in the system, a distribution that will then lead to the formation of a stable opposing coalition which will nonetheless not be strong enough to have an incentive to use force to weaken the near-dominant state or states. The likelihood of war is minimized, in other words, when power is distributed sufficiently unequally among states that two stable but nearly equal opposing coalitions form.[24]

This conclusion can be used to illuminate post–World War II controversies among students of international relations about the relation between the distribution of power and the likelihood of war. Traditionally, when people have said that power was "balanced," they have meant that it was approximately equally distributed, so that any individual state that might contemplate expansion could be opposed by another state of approximately equal power. In a world of more than two states, this also implied a multiplicity of possible coalitions of states—hence the connection between "balanced power" and the shifting alliances of a "balance-of-power system." The conventional wisdom was that this distribution was the most stable one. This wisdom did not clearly distinguish, however, between stability in the sense of the preservation of the independence of the members of the system and stability in the sense of peace. This confusion was encouraged by the tendency to ignore the fact that while individual states could be opposed by other individual states with equal power, overwhelmingly powerful coalitions of states could form. This helped secure the stability of the system, since it meant that individual states had a long way to go before they were stronger than any possible countercoalition, and therefore hegemony could always be prevented. For precisely that reason, however, states could be uncertain about how far they might expand before they encountered effective opposition, and the fear that if they did not expand they might be defeated by superior coalitions gave them a strong incentive to do so. These facts help explain the two world wars of the twentieth century.[25]

After World War II, three competing ideas about how to secure the peace were influential in Great Britain and the United States: to restore the old system of "balanced power," to create a permanent alliance among the victors that would control the defeated countries, and to use international institutions to create a collective security system. There was instead a conflict among the members of the victorious coalition over how to secure their gains, which led to the Cold War, a development that can easily be understood in the context of the analysis offered. The leaders of the Soviet Union, fearing that their former allies would turn against them and join forces with the countries defeated in World War II, preferred a settlement that left Germany sufficiently weaker—and the USSR sufficiently stronger—that their allies formed a counteralliance to deter the Soviets from any further expansion.[26] The result was a distribution of power similar to the one described.

Because this conflict led to the division of Europe into East and West, it seemed to imply no possibility of a return to a system of shifting alliances among more or less equal states. For those who believed that such a system was the most stable one, this implied a pessimistic conclusion about the future. This interpretation was reinforced by the common view that it was such a polarization that had helped cause World War I. Instead of war, however, the polarization that accompanied the Cold War led to what John Gaddis has dubbed "the long peace" (Gaddis 1991). This fact supports an alternative view, based on the analysis offered here, that the polarization of the international system did not cause World War I but that its incomplete polarization helped make it possible and that it was the prolonged confrontation between East and West that helped keep the peace after World War II.[27]

The distribution of power underlying this confrontation has mistakenly been identified with one in which the two largest countries can defend themselves without allies against any combination of adversaries (one of the common definitions of *bipolarity*).[28] This has made it difficult to explain the fact that "more than anything else, the Cold War between the United States and the Soviet Union has been a competition for allies" (Walt 1987, 3). However, this behavior can easily be understood in light of the argument presented here. It is precisely because it seemed so plausible that U.S. security did not depend on allies that so many resources were expended by the American government during the Cold War to demonstrate that it considered the defense of its allies to be a vital interest. This is consistent with the thesis that what is commonly known about the distribution of military capabilities is not enough to determine with certainty the vital interests of states, but a distribution of capabilities such as this one facilitates the exchange of private information about what those interests are. Thus the emphasis of the U.S. government on deterrence during the Cold War was not simply the result of the development of nuclear weapons—a fact that illustrates the close connection between my theme and deterrence.[29]

Consider, on the other hand, the problem of deterring Iraq from its recent conquest of Kuwait. Even if it had been common knowledge that the U.S. government was committed to preventing any state in the Middle East from becoming unacceptably powerful, no one could have been certain that the United States would use force to liberate Kuwait if it were conquered by Iraq. And uncertainty about U.S. preferences gave the government of Saudi Arabia an incentive to avoid provoking the government of Iraq and thus to avoid stating clearly that Saudi Arabia would cooperate in opposing it. Moreover, because the United States had wanted the cooperation of Iraq in opposition to Iran, it had helped Iraq acquire sufficient military power that Saddam Hussein could believe that Saudi Arabia would not dare oppose it. But without the cooperation of Saudi Arabia, the liberation of Kuwait would have been impossible. Thus if, prior to the invasion of Kuwait, the United States had succeeded in convincing the government of Iraq that it had turned against it without convincing Saddam Hussein that the government of Saudi Arabia was prepared to oppose it as well, this would only have given Saddam Hussein an added incentive to absorb Kuwait. This, on a small scale, is an illustration of how shifting coalitions make deterrence more difficult.[30]

SUMMARY AND CONCLUSIONS

It is often said that war represents the failure of diplomacy. As Blainey pointed out, one could just as easily say that diplomacy represents the failure of war: the problem is to understand why states sometimes choose one and sometimes the other (1988, 292). And to understand the relation between the distribution of power and the likelihood of war, we must understand how the distribution of power affects this choice. This is a much more complicated question than it is commonly made out to be, for two reasons. First, war and diplomacy are not just alternatives: the point of war is to influence the outcome of negotiations, so war can be a way of conducting diplomacy. Thus to understand the relation between war and diplomacy, one must understand the relation between the use of force and bargaining. Second, conclusions about the effect of the distribution of power on the probability of war that may seem plausible in a world of two states do not necessarily apply to historical international systems in which there have always been more than two states.

The primary function of force in bargaining is to improve one's bargaining position by increasing the costs of disagreement for one's adversary. Force can serve this function even if both sides can employ it against each other, as long as one expects to suffer proportionally less than the other (Pillar 1983, 144–95). However, force is much more effective if it can be used unilaterally, so that another function of force is to disarm one's enemy, thereby preventing him from using force against one while one uses force against him. All wars are to some extent contests in punishment, since all wars are to some degree costly and unpleasant. Interstate wars have primarily been contests in disarmament, however, in which each side seeks to acquire the ability to punish the other unilaterally by first destroying its military forces.

Counterforce contests, if fought to the finish, must end in the disarmament of one side or the other, or in stalemate. "Victory" and "defeat" in this context, then, refer to the outcome of a counterforce military contest and not to the achievement of the political objectives that may have motivated it, and "power" means whatever assets states have that affect the probability that they will win such a contest. The fruits of victory in this sense may be disappointing, and so power in this sense may be worth much or little. Wars, however, need not be fought to the finish, so that another possible outcome of war is that it may lead to a negotiated settlement before either side wins (or before it becomes certain who the eventual winner will be). Thus states may choose to fight even if the expected outcome of fighting is a military stalemate, if each is optimistic about the effect of a stalemate on the willingness of the other to compromise.

States must choose, then, between negotiating without fighting and fighting followed by negotiations. Any agreements that states reach must be self-enforcing, however, in the sense that implementing an agreement cannot be expected to enable one of the parties to overturn it and enforce a still more favorable agreement. Thus if the terms of an agreement affect the relative power of the parties to it (e.g., if what is in dispute is the distribution of territory and the military power of each is positively related to the amount of territory it controls), then compromise agreements accepted in lieu of war may not be enforceable.

If agreements are enforceable, it is most likely that there will be an agreement that both antagonists prefer to war if their expectations concerning the likely outcome of war are consistent. This is the main reason for believing that the likelihood of war between two antagonists varies inversely with the degree of inequality in the distribution of power between them, because, for the reasons given, it seems plausible that inequality of power makes consistent expectations of the outcome of war more likely.[31]

However, much of the literature on international politics assumes not only that war is influenced by the distribution of power but also that war affects the distribution of power—and therefore that the outcome of one war will affect the expected outcome of subsequent wars. This implies that compromise agreements are not enforceable, so that if one state prefers the expected outcome of fighting to the status quo, war can be avoided only if the other prefers to surrender without fighting.[32] Therefore in a two-country world if both sides are equally likely to use force when faced with the same probability of winning, the likelihood of war is least if power is distributed equally (so that the likelihood that either will prefer war to the status quo is least) or if power is distributed very unequally (so that, for the weaker side, the probability of winning is outweighed by the expected costs of fighting).[33]

When there are more than two states, expectations of the outcome of wars depend on who will participate in them as well as on the distribution of power, and expectations about who will participate will be influenced by private information about how states define their interests. All states can be expected to have an incentive to defend their independence, however, so that the more a state's independence is threatened by another state or coalition of states, the more consistent should be everyone's estimates of the probability that it will join an opposing coalition. This leads to the conclusion that in a world of three or more states the likelihood of war is minimized when power is distributed sufficiently unequally that at least one state (or, if there are more than three states, a coalition of two states) is considered unacceptably close to dominance by the other states in the system, who therefore form a stable opposing coalition with nearly equal power.[34] This conclusion clarifies the relation between controversies about whether equal or unequal power distributions reduce the probability of war and controversies about whether bipolar or multipolar international systems are more stable.

The relation between the distribution of power and the likelihood of war depends, then, on whether the terms of possible compromise agreements that might be accepted in lieu of war influence the balance of power between the antagonists and therefore influence the probability that the agreement will be enforced—and if they do, it depends on how many states' interests will be affected by the outcome. Thus there can be no general answer to the question of what relation one should expect between the distribution of power and the likelihood of war, and reasoning that may be appropriate for understanding conflicts among the great powers during certain historical periods may not be relevant to conflicts that have only local or regional significance or to other periods. Note that this conclusion would hold even if the only factor affecting the likelihood of war were the distribution of power, and power (i.e., all the publicly known attributes of states that affect the probability of their success in counterforce military contests) could be unambiguously measured.

In fact, of course, many other factors besides the distribution of power affect the likelihood of war, and their effect may be much greater. Moreover, the way in which decision makers translate the distribution of such commonly known assets of states as population and wealth into probability estimates of the outcomes of various possible wars may be difficult to discern and quite variable.[35] Thus it is not surprising that empirical tests of competing claims about the effect of the distribution of power on the probability of war have been inconclusive.

Finally, this discussion has two important implications for the use of formal models in investigating theoretical issues in the study of international politics: (1) the issues discussed here are more complicated than they are often made out to be and are therefore difficult to explore without the aid of formal models, but (2) in constructing such models one must be careful about the assumptions one makes. If the role of a formal model is to provide a counterexample to some general assertion in the literature, the realism of its assumptions is not very important.[36] But if a model is to tell us something we did not already know about some historical situation, it is important that its conclusions not require that states in that situation either be able to do things they could not in fact do or unable to do things that they could in fact do. Many models assume that prior to war, any agreements states might make are enforceable but that once war begins no further negotiation is possible—and then proceed to analyze the implications of these propositions in a world of two states.[37] As the current civil war in Yugoslavia graphically illustrates, these assumptions are not as innocuous as they appear.[38] And even scholars who are likely to notice the significance of assuming that agreements between states are enforceable (e.g., those who identify themselves as "realists") tend to overlook the fact that the nonenforceability of agreements does not have the same significance in a world of more than two states that it has when there are only two.[39]

Notes

An earlier version of this article was presented at the annual meeting of the American Political Science Association, 1992. I would like to thank James Fearon and Robert Powell for helpful conversations and correspondence on the subject.

1. For a recent discussion with citations to the literature, see Fearon 1992.

2. It is sometimes suggested that this conclusion requires the assumption that all states are in fact interested in territorial expansion. However, this assumption is clearly stronger than necessary.

3. Both these arguments assume that it is impossible to decrease one state's likelihood of success in attacking without increasing the other's. Obviously this need not be true. In medieval times building a castle diminished the likelihood that one's enemies would be successful in attacking without increasing one's own chances of success in attacking one's enemies. Natural geographical barriers as well as great distances can have the same effect. This is one basis for the view that whether technology favors the defense over the offense is more important in influencing the likelihood of war than the balance of power. One cannot count on the appropriate technology, however, and given some prevailing technology, the relative size of military forces may still be important.

4. A similar idea can be found in Timasheff 1965.

5. The latter method is the basis for the Rubinstein model of bargaining. For an accessible recent discussion of the relevant theoretical issues, see Kreps 1990. For an attempt to apply this model to the choice between fighting and bargaining, see Powell 1992.

6. One of the difficulties even with formal models of bargaining that assume incomplete information is that they predict that bargainers will reach agreement much sooner than they often do in fact (Powell 1992).

7. Consider Saddam Hussein's current reputation for compromise.

8. Because wars are contests between collectivities, there is a difference between postwar negotiations in which the defeated country retains a government and negotiates as a single political entity and a situation in which the victor occupies the defeated country and deals directly with the population. For discussions of bargaining between victor and vanquished, see Albert and Luck 1980; Kecskemeti 1958; Sharp 1990. For a discussion of negotiated settlements of wars using cooperative bargaining theory, see Pillar 1983. For a discussion of negotiated settlements of civil wars, see Wagner 1993a.

9. One must distinguish between victory/defeat on one battlefield or in one theater and strategic victory/defeat. The Japanese defeated the U.S. forces at Pearl Harbor. Had the United States not retaliated and acquiesced in Japanese expansion in the Far East, this would have been because it chose not to, not because it was unable to, and in that case, it would have been considered a negotiated settlement between the United States and Japan *influenced* by a military engagement, not simply a military victory by Japan. One could say the same about a decision by the United States and England not to invade the European continent after the defeat of France by Hitler's Germany or a decision by the United States and its allies to acquiesce in the conquest of Kuwait by Iraq. This is relevant to understanding what Mearsheimer calls a "limited aims strategy" (1983, 23–66; see also Clausewitz 1976, 69, 90–99).

10. This is important to bear in mind in thinking about the impact of nuclear weapons on the likelihood of war. The combination of nuclear weapons technology and ballistic missiles changes the relation between the counterforce and punishment uses of organized military force and therefore the significance of the *balance of power* in the sense of that term employed here: because of ballistic missiles, it is difficult to disarm one's opponent completely, but adversaries are still able to do massive amounts of damage to each other (Schelling 1966, v–vii, 1–34). However, conventional wars have always been costly when neither side was able to disarm the other; and one cannot be certain that states with nuclear weapons will be dissuaded from engaging in the sort of contest in punishment that military stalemates have always made possible.

11. Blainey actually seems to defend a stronger thesis—that inconsistent expectations of the outcome of war are a neces-

sary condition for fighting. Fearon (1992) analyzes a model illustrating the falseness of this claim, since even with consistent probability estimates of the outcome of war bargainers may not be able to reach an agreement they both prefer to fighting if their evaluations of the costs of fighting are not common knowledge. The same point is made more informally in Wagner 1993a. Because Fearon assumes consistent probability estimates, in his model the probability of war is completely unrelated of the balance of power.

Although Mearsheimer (1991) asserts that war is least likely when power is equal, his book, *Conventional Deterrence*, provides evidence that supports this version of the opposite view (idem 1983). In this book Mearsheimer presents several case studies supporting the thesis that war is most likely when decision makers believe they have a strategy that will enable them to win quickly without much cost—a blitzkrieg strategy (pp. 23–66). But he points out that "when a potential attacker enjoys an overwhelming force advantage, the importance of discovering and successfully implementing the optimum military strategy is not as great as when the forces are essentially balanced" (p. 65). This is consistent with the idea that when forces are equal, estimates of the likelihood of success are most likely to be influenced by factors whose status is private information.

12. For an example of this sort of problem, consider the worries of many Israelis about the effect of territorial compromises with the Palestinians on the ability of Israel to resist subsequent Palestinian demands. This may be what people have in mind when they say that because international conflict is a struggle for power, it is a zero-sum game. This is not a zero-sum game, however, since both adversaries would be better off if they could agree without fighting; the problem is that no agreement is an equilibrium.

13. For this to be true, any redistribution must reduce the beneficiary's probability of losing or its expected costs of war enough to compensate for the fact that the redistribution also reduces the beneficiary's net gains from winning. But it is unlikely to be common knowledge whether this is true or not, so that inconsistent beliefs about these effects may serve to inhibit compromise even if it is in principle possible. James Fearon has pointed out to me that if delays are expected between rounds of negotiation then with sufficient discounting of future benefits states might agree to settlements that led to piecemeal surrenders to territory. This provides a possible interpretation for what are called "salami tactics." I shall assume that states will not behave in this way.

14. One might object that this admittedly highly simplified depiction of the relation between the distribution of power before and after wars overlooks the fact that military resources are destroyed by war. However, as the examples of Japan and Germany illustrate, a war may destroy a country's current military capabilities without destroying its military potential—hence the significance of territorial expansion for the postwar balance of power.

15. Thus the victim has an incentive to make cheating easier by not resisting every member of the enemy coalition equally (Wagner 1986). For a historical example of this, see Kecskemeti's account of the debate among German leaders about what strategy to follow once they became certain of Germany's ultimate defeat in World War II (1958, 127–54). See also Koerner and Goemans 1990.

16. For a fuller discussion, see Wagner 1986. Niou and Ordeshook arrive at the same conclusion by assuming that the victim can protect itself from elimination by preemptively transferring enough resources to one of the members of the attacking coalition to give it 150 units and thus prevent any further transfers (Niou and Ordeshook 1990; Niou, Ordeshook, and Rose 1989). This is an extremely implausible assumption, however, not because voluntary transfers are impossible (they clearly are not) but because the assumption implicitly forbids any possible counteroffer to such a transfer. If counteroffers are possible, one must show why an ally could not prevent its partner from accepting such a transfer by threatening to ally with the victim instead. Clearly the victim would prefer such an offer to making the transfer, and the ally who would be frozen out by such a transfer could be no worse off if he made it. Thus it appears that in any plausible noncooperative model of this situation in which counteroffers are not prohibited, all states desire more resources, and there is no uncertainty about the outcomes of wars, the preemptive transfers assumed by Niou and Ordeshook would not be equilibrium outcomes.

17. I shall continue to assume that military victory gives the winning side control over all the resources of the losing side and therefore that uncertainty only characterizes expectations of the outcome of combat. However, it is important to bear in mind that military victory only gives the victor the ability to use force unopposed and does not deprive the vanquished of all bargaining power. Thus victory does not necessarily mean that the winner can exploit all the resources of the loser or even that it will be strengthened by winning. Therefore there may be uncertainty about the effects of military victory as well as about whether to expect it. For a fuller and more rigorous analysis, see Wagner 1994.

18. Thus the unilateral transfers assumed by Niou and Ordeshook become more relevant when not all states desire to expand and there is uncertainty about the distribution of power.

19. Stephen Walt (1987) assumes that balance-of-power theory is inconsistent with the formation of defensive coalitions that are much more powerful than their adversaries and that such indexes as size, population, and gross national product are adequate measures of the power of states. Thus he concludes that the sizes of the alliances that formed against Germany and Japan and against the USSR are inconsistent with traditional balance-of-power theory. Neither assumption is warranted, however. See, e.g., the sections entitled "The Uncertainty of the Balance of Power" and "The Unreality of the Balance of Power" in Morgenthau 1967, 196–205.

20. Walt (1987) distinguishes between *balancing* and *bandwagoning*. If this distinction is as general as he claims, it ought to be applicable here, but in fact its implications are not clear. If the third state joins with the first, is that bandwagoning? We have seen that it is one way of trying to make itself more secure. Similarly, a concession by the second state to the first would increase the third state's incentive to defend it: Would it then be balancing or bandwagoning? These questions illustrate the fact that the distinction between balancing and bandwagoning is unclear unless one state is on the verge of dominance. This helps explain the motivation for Walt's ad hoc distinction between balancing against "power" and balancing against "threats."

21. Niou and Ordeshook (1990) discuss collective security as a nonstationary equilibrium in a noncooperative model of balance-of-power system. However, their model assumes the possibility of preemptive unilateral resource transfers, no uncertainty about the outcomes of wars, perfect information (so that it is always known which state deviated from the equilibrium), and a preference for expansion on the part of all states. Without these assumptions it is doubtful that the punishment strategies necessary to sustain a nonstationary equilibrium of this sort could be supported.

22. Consider, for example, how England might have shown, prior to World War I, that it would ally with Russia to prevent the expansion of Germany in case of a general war on the continent but would not support Russia if it tried to make gains at the expense of Germany and Austria.

23. Consider the question in the context of U.S.–Soviet cooperation in the war against Hitler's Germany and the U.S.–Soviet conflict over the future of Germany that followed.

24. For a fuller discussion, see Wagner 1994.

25. Kenneth Waltz was perhaps the first to point out this problem with international systems in which power was distributed more or less equally. However, he mistakenly identified it with the problem of securing contributions toward the supply of a collective good (Waltz 1979, 164–65). The real difficulty is instead the fact that until a state is close to dominance, opposing its expansion may *not* be a collective good. Therefore the problem diminishes not when multiactor international systems are replaced by two-actor systems

(which has never happened) but when states are powerful enough to attract organized opposition before they attempt to expand (Wagner 1993b; see also Koerner and Goemans 1990).

26. This interpretation of the Cold War implies that ideology was important not because it influenced the objectives of Soviet leaders but because it affected their expectations of the future behavior of their allies and because of the effect of the Stalinist system on the confidence of Soviet leaders in their ability to control large amounts of territory.

27. With respect to World War I, compare the coalition that won the war with the alliances that preceded it. Moreover, while Britain's commitments (which reflected its interests) turned a continental war into a world war, uncertainty about what Britain would do helped make the war more likely (Levy 1990/91). Of course, during the Cold War not every state joined in opposition first to the Sino–Soviet alliance and then to the Soviet Union alone, and there was a great deal of debate about how large an opposing coalition was necessary. This is not surprising, given the uncertainty involved in estimating the distribution of power.

28. Many writers assume that since the United States and the USSR did not need allies there were only two "great powers", and therefore that the international system during the Cold War was equivalent to one with only two states. This has led to further confusion by mingling together controversy about the significance of the number of actors with controversy about the effect of the distribution of power among them. For a fuller discussion of the various meanings of the word *bipolarity* and their relation to the analysis offered here, see Wagner 1993b.

29. In explaining the behavior of the American government during the Cold War, it is important to bear in mind that having to defend Europe from even very small military encroachments was considered almost as big a disaster as trying to defend it and failing. Thus the objective of policy was not simply to be able to defend one's interests with some acceptable probability of success, but to minimize the probability of having to do so. This was a much more ambitious objective than states had pursued prior to World War II.

30. I do not mean to suggest that the U.S. government made no mistakes in dealing with Iraq but only that criticisms of its behavior after the fact have understated the difficulty of the problem, in part because they assume that the United States alone, by acting differently, could have deterred the invasion of Kuwait. For a fuller discussion, see Stein 1992.

31. Bear in mind that "power" in this context means all the resources of states that influence the probability of their success in a counterforce military contest and that are matters of common knowledge.

32. See, however, the qualifications in n. 13.

33. However, since one side can become nearly certain of its eventual defeat before a war is fought to the finish, wars can still be ended by negotiation even though they could not be prevented by them. Moreover, when power is distributed equally, the role of private information in influencing expectations of the outcome of war is increased, so that if war occurs the two sides are likely in fact to have inconsistent expectations of its outcome. Blainey (1988) confused uncertainty with inconsistent expectations and thus falsely inferred that because wars can lead to negotiations and because prior to war both sides tend to be optimistic about its income, wars are always caused by inconsistent expectations and thus that inequality of power always reduces the likelihood of war.

34. In evaluating the significance of this conclusion, one must bear in mind the fact that it does not tell us how likely war is when its likelihood is minimized. Moreover, I have throughout assumed that the distribution of power could be changed only by wars and alliances. As a result first of the democratization and then of the industrialization of war, however, since the French Revolution the distribution of power has been subject to very large exogenous changes. When no state or pair of states is close to dominance, such changes magnify the difficulty states experience in developing consistent expectations of each other's behavior. When one or two states are close to dominance, however, an expectation of an exogenous change that would strengthen them can provide a motivation for the other states to wage a preventive war, since even a 50% chance of defeating the near-dominant state or states before they become stronger may be preferable to a much smaller chance of defeating them later.

35. In particular, there is no reason to believe that if the power estimates of the Correlates of War Project are converted into percentages they can be used to represent the probability estimates of historical decision makers.

36. See, e.g., Fearon's (1992) use of a formal model to provide a counterexample to a claim in Blainey 1988.

37. See, e.g., the model analyzed in Bueno de Mesquita and Lalman 1992. There are other, stronger, assumptions also embedded in the Bueno de Mesquita and Lalman model, among which is the assumption that the state making a demand has a first-strike advantage if it chooses to fight but surrenders that advantage to the victim if it chooses to negotiate.

38. The argument presented here is relevant to one of the issues in the current debate about how outsiders might help end the war in Yugoslavia—whether offering to enforce a settlement or increasing the power of the Bosnian forces would make a settlement more likely. Suppose outside intervention is expected to prevent the Serbs from defeating the Bosnians and therefore lead to a military stalemate. If both sides were confident that any settlement they reached would be enforced, then outside involvement leading to a stalemate might lengthen the war if each side were optimistic about the other's willingness to compromise on the terms of such a settlement in order to avoid continuation of the conflict. On the other hand, if (as seems more likely) neither side is confident of outside enforcement and any territorial settlement will influence the balance of power between the combatants in subsequent conflicts, then expectation of a stalemate might lead the Serbs to accept an armistice that preserves the existing distribution of territory. However, any subsequent change in the distribution of power between the two sides might lead to a reopening of the conflict. In the meantime, the war continues because the Serbs are too confident of winning and the Bosnians not sufficiently certain of losing to stop fighting. Thus, if the war cannot be ended by diminishing the expectations of the Serbs, it can only be ended by diminishing the expectations of the Bosnians.

39. On this point, see also Snidal 1991.

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STRATEGIC VOTING EQUILIBRIA UNDER THE SINGLE NONTRANSFERABLE VOTE

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Previous investigations of strategic voting equilibria in mass electorates have looked only at single-member districts. I shall investigate such equilibria in multimember districts operating under the single nontransferable vote system. What appear to be the most natural equilibria conform to the M + 1 rule, according to which strategic voting in M-seat districts produces exactly M + 1 vote-getting candidates in equilibrium, any others having their support totally undercut. This result provides the beginnings of a formal underpinning for Reed's recent extension of Duverger's Law to the Japanese case. The model also generates specific and empirically testable hypotheses concerning the exceptions to the M + 1 rule that one ought to expect in equilibrium. I test these hypotheses with Japanese data. Finally, the model also reveals a type of strategic voting that is specific to multimember districts. I use Japanese data again to explore the empirical importance of this kind of strategic voting.

For as long as voting procedures have been used to decide important and controversial issues, there have been legislators and electors willing to vote strategically. Theoretical interest in strategic voting dates at least to Pliny the Younger (see Farquharson 1969) and probably earlier. I shall build on rather more recent and formal treatments of the strategy of voting: those framed in the decision-theoretic and game-theoretic traditions. Most of this work has appeared in the last 25 years and focuses on the behavior of legislators (e.g., Austen-Smith 1987; Banks 1985; Farquharson 1969; McKelvey and Niemi 1978; Miller 1980; Ordeshook and Schwartz 1987; Shepsle and Weingast 1984). I shall focus on the other, less well trodden area of research into strategic voting, that dealing with the behavior of voters in mass elections.

I deal in particular with elections satisfying the following three criteria: (1) each voter casts a single vote (for a candidate, not a party), (2) there are $M \geq 1$ seats to be filled in the district in which the election is held (the model focuses on one district at a time), and (3) plurality rule determines who wins the available seats (so the candidates with the M highest vote totals win election). Two electoral systems satisfy these criteria: the Anglo–American first-past-the-post system, in which $M = 1$, and the Japanese single nontransferable vote (SNTV) system, in which $M > 1$. The first of these systems is, of course, widely used; the second is currently employed at the national level only in Japan and Taiwan (and the Japanese are, as of this writing, in the midst of changing their electoral system).[1] Although structurally very similar, the difference in district magnitudes between the two systems leads to substantial practical differences in systemic performance and party strategy. Nonetheless, the two systems are alike in their strategic voting equilibria, as will be shown.[2]

Although my primary interest here is in the theoretical nature of strategic voting under SNTV (I shall consider the Anglo–American system to be SNTV with $M = 1$), some important and empirically testable implications of the theory arise along the way. These will be duly explored.

First, I shall review the previous formal literature on strategic voting (leaving the vast informal literature largely untouched). Then, I shall describe an *M*-seat *K*-candidate model of strategic voting under SNTV. Next, I shall show that in equilibrium, no votes are wasted at any margin, which entails both that identifiably weak candidates are deserted by their supporters (the feature upon which previous work has focused) and that identifiably strong candidates are deprived of their excess supporters. Finally, I shall explore the empirical usefulness of the model, using Japanese data as an illustration, and conclude.

PREVIOUS RESEARCH

The most widely recognized form of strategic voting occurs in single-vote plurality-rule elections held in single-member districts. As early as 1869, Henry Droop (an English advocate of proportional representation) recognized the basic logic: "As success depends upon obtaining a majority of the aggregate votes of all the electors, an election is usually reduced to a contest between the two most popular candidates. . . . Even if other candidates go to the poll, the electors usually find out that their votes will be thrown away, unless given in favour of one or the other parties between whom the election really lies" (quoted in Riker 1982, 756). Droop also saw an important systemic consequence of this district-by-district tendency toward strategic voting: the political nation was divided into two large catchall parties, rather than into many smaller and better-defined parties (see Riker 1982, 756–57). He thus anticipated what we now know as Duverger's Law—the proposition that electoral laws mandating the use of plurality rule in single-member districts tend to produce two-party systems (Duverger 1955; Riker 1982).

Formal mathematical study of strategic voting in the last 20 years has had two stages: an early decision-theoretic stage and a more recent game-theoretic stage. The decision-theoretic perspective on strategic voting (see McKelvey and Ordeshook 1972) is essentially the same as Droop's: some voter, whose favorite candidate has a poor chance of winning, notices a personal preference between the top two candidates; the voter then rationally decides to vote for the most preferred of these top two competitors, rather than for his or her overall favorite, because the latter vote has a much smaller chance of actually affecting the outcome than the former. The decision-theoretic approach adds to common sense not just greater precision about the assumptions implicit in Droop's reasoning (e.g., the probability not of victory but of ties and near ties matters directly) but also greater generality: the basic model has been extended to illuminate strategic behavior in multimember districts (Cox 1984), approval voting elections (Niemi 1984), and a variety of other electoral systems (Hoffman 1982).

Nonetheless, decision-theoretic analyses, both formal and informal, still deal essentially with a single voter in analytic isolation. The logical next step is to consider whether strategic voting by some voters makes such voting by others more or less likely. In particular, suppose a close third-place candidate in a single-member district begins to lose the support of his least committed followers (those who prefer him only slightly to one of the two frontrunners). This erosion of support will, if known (perhaps through polls), lead voters to reduce their estimates of the candidate's chances. But as the candidate's chances are seen to fall, some of his slightly more committed followers may abandon ship for one of the frontrunners. The process might, in theory, continue until the candidate was left with no support.

This line of thinking is game-theoretic. It essentially asks how much strategic voting there is in equilibrium. Should one expect that third-place candidates will always lose all of their support because of strategic decisions among their followers? Or are there general conditions under which this erosion of support is fairly limited or even negligible?

I addressed these questions earlier in the context of a model (similar to that in Ledyard 1984) in which three candidates compete for a single seat under the usual Anglo–American rules (Cox 1987). The key assumptions of the analysis were that all voters are instrumentally rational (i.e., they care whom they vote for only insofar as it affects the outcome of the election), that voters have incomplete information about each other's preferences over outcomes, and that all voters have "rational" expectations. I showed that in almost all equilibria some voters vote strategically and that the marginal impact of strategic voting was to decrease the effective number of parties (Laakso and Taagepera 1979).

Palfrey (1989), exploring essentially the same model, was able to characterize its equilibria in terms of candidate vote shares, showing that they fall into two classes: Duvergerian equilibria (in which the level of strategic voting is such that the support of all but two of the candidates is undercut completely) and non-Duvergerian equilibria (in which two or more candidates are so nearly tied for second that the voters cannot decide which one to discount, leaving more than two significant candidates in the field). Palfrey believed that the non-Duvergerian equilibria were razor's-edge phenomena and hence that he had provided an internally consistent explanation of Duverger's Law in terms of strategic voting alone. Myerson and Weber however, have shown that this belief is incorrect (1993, 106). Although the non-Duvergerian equilibria do seem unusual because of their requirement that two or more candidates be virtually tied for second, they are not unusual in the mathematical sense of being nongeneric. The intuition behind them is roughly as follows. Suppose two leftist candidates (e.g., Charles Goodell and Richard Ottinger) and one rightest (e.g., James Buckley) are competing for a single post (one of the U.S. Senate seats for New York in 1970). The rightist is ahead, the two leftists trailing but close to one another. Under these conditions, leftist voters will have a hard time coordinating on one of the leftist candidates, and a non-Duvergerian result can (and did) ensue. This failure to coordinate, moreover, does not require nongeneric conditions on the distribution of voter types, such as that the two leftists have exactly the same number of voters ranking them first. If more voters rank leftist A first than rank leftist B first, for example, then the two can still end in a dead heat (or sufficiently close) if it is expected that B has a larger chance of being tied for first with the rightist than does A. Such beliefs appear unusual in that they require the "objectively weaker" candidate B to have a better chance of being tied for first than the "objectively stronger" candidate A, but they are not ruled out by the Bayesian equilibrium concept. At present, therefore, Duverger's Law cannot be derived exclusively from analyses of strategic voting equilibria.[3]

Myerson and Weber advance a model of voting equilibria applicable in a wide range of single-winner electoral systems—not just ordinary plurality rule but also approval voting, Borda's method of points, and many other systems as well. The main difference between their approach and Palfrey's is that they assume as an axiom something Palfrey derives endogenously, roughly, that candidates generally expected to place third or lower in the poll are much less likely to be tied for first than candidates generally expected to place first or second in the poll.

I shall try to extend the study of strategic voting to cover multimember districts. I adapt Palfrey's model to the multimember context, holding all other institutional features (one nontransferable vote per voter, seats allocated by plurality rule) constant. At the same time, some features of the Myerson and Weber (1993) and Hoffman (1982) approach are incorporated as well.

A *K*-CANDIDATE *M*-SEAT MODEL

There are K candidates, indexed by $\mathbf{K} = \{1, \ldots, K\}$, competing for M seats. The M candidates placing highest in the poll win the available seats. If two or more candidates are tied for Mth in the poll, then the tie is broken equiprobably. After any ties have been broken, the *outcome* of the election is a set of M elected candidates. The set of all possible outcomes, denoted Ω, accordingly has $\binom{K}{M}$ members. Candidates are nonplayers in the model; they take no actions and are simply "entities with fixed characteristics about which voters might care."

There are n voters, each possessing a (strict) preference ranking over the possible outcomes of the election. The analysis is simplified, without altering the basic point, if we assume that voters' preferences over outcomes are additively related to their preferences over candidates, that is, each voter i is able to assign Von Neumann–Morgenstern utilities to each candidate—$u_{i1}, u_{i2}, \ldots, u_{iK}$—such that outcome $\alpha \in \Omega$ is preferred to outcome $\beta \in \Omega$ if and only if $\Sigma_{j\in\alpha} u_{ij} > \Sigma_{j\in\beta} u_{ij}$, with indifference obtaining if and only if the summed utilities are equal. Voter utilities can be rescaled in the standard fashion so that the most preferred candidate yields a utility of 1, the least preferred candidate, a utility of 0. After this rescaling, voter i's preferences (or voter i's *type*) can be described by the vector $u_i = (u_{i1}, \ldots, u_{iK})$, an element in the set $U = \{(u_1, \ldots, u_K)\colon \max\{u_j\} = 1 \;\&\; \min\{u_j\} = 0 \;\&\; u_j = u_k \text{ only if } j = k\}$.

The relative frequencies of the different possible types of voter preference are given by a cumulative distribution function F defined over U. I assume that all possible preferences are represented in the electorate, possibly with vanishingly small probability (formally, the support set of F is U). I also assume that the probability two randomly sampled voters have identical preferences is negligible (formally, F has no mass points).[4]

Each voter chooses a vote in order to maximize expected utility, something that depends not just on the voter's preferences over candidates but also on his or her *expectations* about how well each candidate is likely to do. These expectations are formalized here as a vector $\pi_i = (\pi_{i1}, \ldots, \pi_{iK})$, where π_{ij} equals i's (subjective) probability that a randomly selected voter will vote for candidate $j \in \mathbf{K}$. Equivalently, π_{ij} is the expected proportion of the electorate who will vote for j, according to i. Given preferences (u_i), expectations (π_i), and knowledge of the number of voters (n), voter i faces a standard decision problem, whose details are given in Appendix A. The solution to i's problem (i.e., the set of votes that maximize expected utility, given u_i, π_i, and n) is denoted $V(u_i; \pi_i, n) \subseteq \mathbf{K}$.

The model is completed with two further assumptions whose joint effect is to restrict the nature and consistency of voter expectations. First, I assume that the distribution of voter preferences, F, is common knowledge. Second, I assume that voters' expectations are publically generated (e.g., by polls and newspaper analysis of the candidates' chances), so that diversity of expectation among the electorate is minimized. In the discussion that follows, I take this notion to the logical extreme and assume that every voter has the same expectations: $\pi_i = \pi$ for all i. This assumption can be replaced with a less stringent one, however, namely, that the expected *order* of finish of the candidates is the same for all voters, in the sense that they all agree on which candidates are *trailing* (expected vote shares that put them strictly below (M + 1)th place), which are *leading* (expected vote shares that put them strictly above Mth place), and which are *marginal* (neither trailing nor leading).[5]

Given these two postulates, the maintained assumption of voter rationality implies a certain consistency between F and π in equilibrium, for, not all expectations π are "rational" in light of the voter's knowledge of the distribution F of voter preferences. Suppose (to take a four-candidate example) that some voter thought π equaled (.25, .25, .25, .25), so that a randomly selected voter was equally likely to vote for any of the candidates. This expectation is clearly not consistent with a distribution of voter preferences in which the proportion of voters ranking candidate 1 last exceeds .75, since voting for 1 is a dominated strategy for such voters.

More generally, if π is a publically generated expectation common to all voters, and the number of voters is common knowledge, then each voter can simply compute the optimal votes $V(u; \pi, n)$ of all other voter types u. Then, knowing the distribution of voter types, the voter can calculate the proportion of the electorate who, given π and n, will vote for each candidate.

To see this, let $H_j(\pi, n) = \{u \in U\colon j \in V(u; \pi, n)\}$ be the set of all voter types who will vote for j, given π and n. Then the probability that a randomly sampled voter will vote for j is simply $\int_{H_j(\pi, n)} dF$.[6] If $\int_{H_j(\pi, n)} dF$ is not equal to π_j, then the original expectations are not tenable: to continue to believe them entails believing that some other voters will not vote rationally.

This is the gist of the argument for imposing the following "rational expectations" condition on voter beliefs.

RATIONAL EXPECTATIONS CONDITION. *The expectations π are rational with respect to the distribution* F *if, for all* j, $\pi_j = \int_{H_j(\pi, n)} dF$.

The equilibrium conditions for the model are then two. First, every voter votes so as to maximize expected utility, given expectations π (and n); that is, voters of type u vote for a candidate in $V(u; \pi, n)$. Second, the expectations π satisfy the rational expectations condition. These equilibrium conditions are identical to those imposed in a symmetric Bayesian Nash equilibrium, the only differences being the interpretation of the model's elements.

VOTING EQUILIBRIA & WASTED VOTES

What are the equilibria of the model just specified? Letting $N(\pi) = |\{j \in \mathbf{K}\colon \pi_j > 0\}|$ be the number of candidates with positive expected vote shares, there are three main cases to consider: $N(\pi) < M$, $N(\pi) = M$, and $N(\pi) > M$.

Equilibria with $N(\pi) < M$. Equilibria with fewer vote-getting candidates than there are seats to be had do not exist. Suppose, to the contrary, that $N(\pi) < M$ for some expectations π satisfying the rational expectations condition. In this case, perversely enough, *no* voter would actually vote for any candidate j such that $\pi_j > 0$ (i.e., $\pi_j > 0 \rightarrow j \in V(u; \pi, n)$ for no u). This follows because, given π, n, and the usual Nash assumption, each voter believes that all candidates j with $\pi_j > 0$ are certain to be elected. Thus there is no incentive to vote for them. On the other hand, voting for the most preferred of the candidates j with $\pi_j = 0$ is, under the same assumptions, certain to elect that candidate (by breaking a tie among K–$N(\pi)$ candidates, all with zero votes, in favor of the selected candidate).

Equilibria with $N(\pi) = M$. Such equilibria do not exist either. Consider a four-candidate, two-seat example to illustrate the point. If $\pi = (0, 0, .5, .5)$ is an equilibrium, then it must be that a voter preferring candidate 1 to 2, 2 to 3, and 3 to 4 would vote for 4 or for 3, given π. But voting for 4 is a dominated strategy for this voter. And given π, voting for 3 is not optimal, for it is possible that 3 will receive less than two votes. In these cases, a vote for 1 is superior to a vote for 3, since it either breaks a three-way tie for second in 1's favor (if 3 receives no votes) or puts 1 into a two-way tie with 3 for second (if 3 receives one vote), both desirable results. In all other cases (i.e., 3 has more than one vote) voting for 1 and voting for 3 yield identical outcomes. Thus, no voter of the specified type will vote for either 3 or 4, and given the assumption that there are some voters of each type, the assumption that π satisfies the rational expectations condition is contradicted.

Equilibria with $N(\pi) > M$. This is the most interesting case, and the one that takes the most work to nail down. Relabel the candidates, if necessary, so that $\pi_j \geq \pi_{j+1}$ for all $j \in K \backslash \{K\}$. Note that with this relabeling, the condition $N(\pi) > M$ implies that $\pi_{M+1} > 0$. Given a distribution F of voter types, I shall say that the expectations π are a limit of rational expectations if and only if, for every $\varepsilon > 0$, there exists an integer N and a sequence $\{\pi^n\}$ of rational expectations such that $n > N$ implies $|\pi_i^n - \pi_i| \leq \varepsilon$; that is, π is a limit of rational expectations if and only if arbitrarily large electorates can have rational expectations that are arbitrarily close to π. The main result is presented in the following theorem and its corollary.[7]

THEOREM 1. *Suppose that either (1)* $0 < \pi_j < \pi_{M+1}$ *for some* $j > M + 1$ *or (2)* $\pi_M < \pi_1$. *Then* π *is not a limit of rational expectations.*

For the proof, see Appendix B. The basic logic of the proof is this: if $0 < \pi_j < \pi_{M+1}$, then candidate j is virtually sure to lose for sufficiently large n, and voting for the most palatable of the candidates most likely to be tied for Mth yields a higher expected utility than voting for j; if $\pi_M < \pi_1$, then candidate 1 is almost certain to win for sufficiently large n, and, again, the instrumental voter's attention turns to those candidates with "asymptotically large" conditional tie-probabilities.

A direct corollary of Theorem 1 is the following.

COROLLARY 1. *If* π *is a limit of rational expectations, then (1)* $\pi_1 = \pi_2 = \ldots = \pi_M$ *and (2)* $\pi_j \in \{0, \pi_{M+1}\}$ *for all* $j > M + 1$.

For $M = 1$, this corollary simply extends Palfrey's earlier three-candidate result to cover an arbitrary number K of candidates. For $M > 1$, the corollary carries two messages about wasted votes, rather than just one. Like Palfrey's previous work, it shows that strategic voting works against trailing candidates (those who fall "too far" behind in the polls). In addition, the corollary shows that strategic voting relieves leading candidates (those who are "too far" ahead in the polls) of their excess votes, reducing them to equality with all other likely winners. Thus votes are wasted neither on weak nor on strong candidates.

The nature of the two processes (winnowing out the weak, equalizing the strong) is slightly different. Voters who find themselves supporting a trailing candidate are more likely to desert that candidate as other voters desert him. There is thus a certain momentum to strategic voting as it affects weak candidates. Voters who find themselves supporting a leading candidate, in contrast, are less likely to abandon that candidate upon hearing of other defections. To illustrate this, consider a contest between three candidates (A, B, and C) in a two-seat district. Suppose A is ranked first by 40% of the electorate and is expected to get all of their votes, while B and C are each expected to get 30% of the vote. In this case, A voters who prefer C to B are tempted to switch their support to C, while A voters who prefer B to C are tempted to switch their support to B. If those who prefer C (B) succeed in bringing A and C (B) into a dead heat at 35% (by abandoning A), then similar action by those who prefer B (C) will be forestalled. If both sides (the A supporters who prefer B to C and those with opposite preferences) abandon A, then the worst outcome for both results. Thus, there is a Chicken aspect to the game: as soon as the existence of excess votes becomes common knowledge, both sides begin "driving" toward abandonment. The question is, Which side will swerve first? The present model, it should be noted, does not capture this Chicken aspect at all well formally, since the equilibrium concept simply assumes away all coordination difficulties. This may be one important reason that the model's predictions regarding the elimination of votes wasted on strong candidates fare so poorly in the empirical analysis.

As in the single-seat case, the corollary divides multiseat equilibria into two classes: (1) Duvergerian equilibria, with $M + 1$ vote-getting candidates, and

(2) non-Duvergerian equilibria, with more than $M + 1$ vote-getting candidates. The Duvergerian equilibria entail a close M-way race for the M available seats, with a single runner-up, all other candidates being reduced to near-zero support. The non-Duvergerian equilibria look the same, except that there are two or more runners-up, whose nearly identical expected vote totals prevent any being winnowed out from the field of viable candidates.

STRATEGIC VOTING AS AN EXPLANATION OF REAL-WORLD DATA

I shall consider the empirical usefulness of the results just sketched. There is no question that instrumentally rational agents of the type stipulated, with rational expectations, will behave in a very precise fashion. But of course it is possible to doubt that real people are entirely instrumentally motivated or that they have rational expectations. And it is a matter of simple observation that the top M candidates in SNTV elections do not have virtually identical vote shares. Taken as is, then, the model's predictions meet with immediate empirical disconfirmation. A reasonable conclusion is that one or more of the model's assumptions are poorly approximated in the real world.

Overly precise predictions are typical of highly abstract models and a typical (often unstated) assumption of theoreticians is that the model's predictions could fairly easily be made more reasonable, without changing their qualitative nature, by adding a bit of "noise" or "friction" to the model. Since the empirical testing of rational choice models has been strongly criticized of late (Green and Shapiro 1993), I will explore what some of the noise to be added might be. Even if adding noise (e.g., noninstrumental voters or voters whose expectations are inconsistent) can in principle produce predictions not obviously false, there is still interest in two questions: (1) Do real-world data conform sufficiently closely to the model's predicted equilibria that one might believe that a model essentially similar to this one (just adding noise) might tally with real-world patterns? (2) Even if the real world conforms to sylized versions of the model's equilibria, are there other explanations that predict the same patterns? I shall examine each of these topics—noise, empirical patterns, and alternative explanations—in turn.

Noise

There are many frictions that would, if introduced into the model, soften its predictions. Here, I shall discuss only two: the introduction of noninstrumental and of "ignorant" voters. Introducing voters who are not purely instrumental but also derive utility from the particular action that they take (abstaining, voting for some candidate) can have various implications. If one assumes that voters bear some cost from voting but that conditional on voting, they do not care whom they vote for except insofar as it affects the outcome, then not much changes. One would have to assume that a fixed proportion of the electorate had nonpositive costs of voting (or something to this effect) in order to generate growing turnout, but after that, the model's results would go through unchanged.

On the other hand, if one assumes that voters care about whom they vote for as well as who wins, then much may change. For such *mixed-motivation* voters, action-contingent utilities would dwarf outcome-contingent utilities as the electorate grows because outcome-contingent utilities are discounted by the probability of a single vote affecting the outcome, whereas action-contingent utilities are not. Thus, a mixed-motivation voter who votes at all votes for the candidates that he or she likes to vote for, regardless of whether this is the candidate that "strategic voting" would dictate supporting. If the entire electorate had mixed motivations, then expectations about how well the candidates were doing would be of negligible importance to voter decisions, and *any* pattern of expectations could be consistent with voter rationality in equilibrium.

If only some voters have mixed motivations, then the result is more interesting. Let Q be the proportion of the electorate that is purely instrumental or outcome-oriented and let $1 - Q$ be the proportion with mixed motivations. In the limit, the latter group will be purely noninstrumental or action-oriented, as noted. Let F be the distribution of preferences in the instrumentally motivated electorate and let P_j be the proportion of the noninstrumentally motivated electorate that prefers voting for candidate j. Then the rational expectations condition becomes $\pi_j = Q \int_{H_j(\pi, n)} dF + (1 - Q)P_j$ for all j. If candidate j is trailing, then j loses all *instrumental* support but retains support among the mixed electorate. Thus, if Q is close to 1, the result does not change much: in equilibrium, strategic voting will reduce weak candidates—if not to zero support, then down to some fairly small minimum (i.e., $(1 - Q)P_j$) of noninstrumental support and will reduce strong candidates down to equality with all other likely winners.

Introducing "ignorant" voters relaxes a different premise of the model, namely, that the identity of trailing, marginal, and leading candidates is common knowledge. The publicness of this knowledge keeps all instrumental voters on the same page of the playbook: they *all* desert the (publicly identified) trailing and leading candidates in order to focus on the (publicly identified) marginal candidates.

One might argue for the reasonableness of the common-knowledge assumption as regards trailing candidates by noting the self-fulfilling character of voter expectations. If every voter *believes* that candidate j is out of the running, then j will in fact *be* out of the running. Moreover, if some voters, who previously intended to vote for j, come to believe that j is behind, they will desert *j*, thereby making it more likely that *j* is behind.

The arguments just given do not really justify *assuming* that the identity of trailing candidates is common knowledge, however. They only justify a belief that in equilibrium, the identity of trailing candidates will probably be common knowledge. To simply assume the common-knowledge condition is similar to assuming that the players in a two-person Battle of the Sexes will coordinate on one of the two pure-strategy Nash equilibria. As regards leading candidates, voter expectations are not even self-fulfilling. If each voter believes that candidate *j* is leading (hence virtually sure to win), then each will abandon *j*, who will in fact finish out of the running.

If who trails and who leads is not common knowledge, then an extra degree of freedom is opened up in the model. In the extreme, the analyst can stipulate (possibly inconsistent) expectations for each voter. This degree of analytical latitude would be enough to make any pattern of aggregate vote returns consistent with some equilibrium of the model. On the other hand, placing limits on the extent to which voters' expectations differed would begin to restore some "bite" to the model's predictions.

These observations motivate asking how voters learn about the candidates' expected vote shares. In the real world, the forces generating common knowledge of candidate chances are polls, news analyses, candidate statements, and other bits of essentially free information. It has to be free information because rationally ignorant voters will not exert any effort to determine who is ahead for the same reason that they will not research candidate positions carefully (Downs 1957). Thus, the extent to which the real world approximates the model's strictures should depend on the availability and clarity of free information regarding the relative standing of the candidates. If voters are exposed to lots of free information (e.g., frequently published polls) that reveals some candidates to be clearly trailing the others and if this information seeps out to a large proportion of the instrumental electorate, then one expects that trailing candidates will be left with not much more than their noninstrumental support. If voters have no information regarding candidate chances (and diffuse priors), then sincere voting is consistent with expected utility maximization, and one does not expect objectively trailing candidates (those who have fewer voters ranking them first) to lose their instrumental support. If (to take a third example) voters have conflicting information regarding candidate chances, then strategic voting by some voters may "cancel out" strategic voting by others, leaving little or no observable impact on the aggregate distribution of votes.

From this perspective, the tendency of candidates trailing in multicandidate races to dispute the accuracy of the polls that show them trailing, to claim to have different results in proprietary polling, and to urge voters to ignore the polls is understandable. All these actions make good sense from the point of view of preventing their last-place status from becoming common knowledge.[8]

Empirical Patterns

The model predicts that strategic voting will produce two main patterns: (1) trailing candidates will be deserted by all their supporters, and (2) leading candidates will be deserted by their excess supporters. In examining how these two predictions fare, I shall look at postwar Japanese electoral data from the period 1958–90. Evidence is fairly good that Japanese voters do strategically desert weak candidates. Steven Reed (1991), in an extensive examination of the postwar Japanese experience with SNTV, finds a clear and increasing tendency for there to be only $M + 1$ viable candidates in each district. This pattern, of course, fits the Duvergerian equilibria identified by the model.

What of the non-Duvergerian equilibria? These equilibria all entail that the first and second losers receive nearly the same number of votes. Thus a theoretically interesting statistic is the *second*-to-*first* loser's vote total ratio (SF ratio). Under Duvergerian equilibria, the SF ratio will be near 0. Under non-Duvergerian equilibria, the SF ratio will be near 1. Thus, if one were to compute the ratio for a number of districts and plot the resulting distribution, one should find a spike at 0 and a spike at 1.

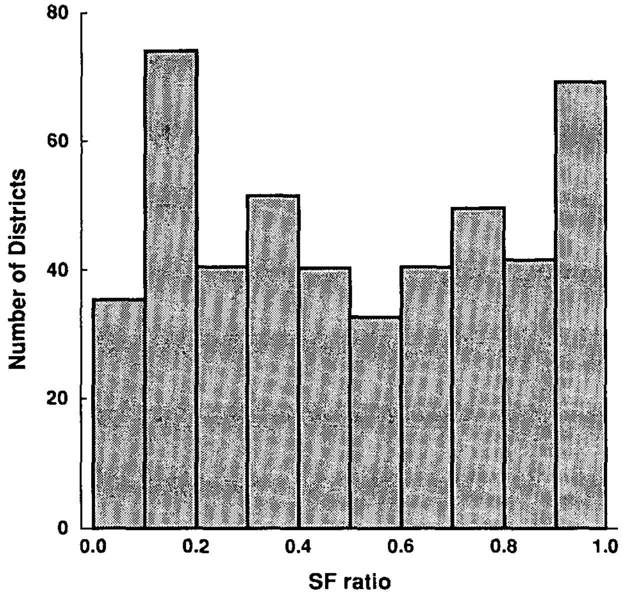
Allowing for some frictions in the model (e.g., some noninstrumental voters, some disagreement about which candidates are trailing or leading, and which are marginal), the prediction is softened. The SF ratio should either be close to 1 (when second losers are so close in the polls to first losers that they do not lose their support due to strategic voting) or close to 0 (when second losers are sufficiently far behind first losers that strategic voting kicks in and they are reduced to their noninstrumental support level, which I assume to be close to 0 for most candidates). The SF distribution, in other words, should be bimodal.

I have tested this bimodality hypothesis empirically in the case of Japan, using district-level electoral returns over the period 1958–90.[9] The procedure, in the case of three-seat districts, was as follows. First, I computed the ratio of the vote total of the second loser (fifth-place candidate) to the vote total of the first loser (fourth-place candidate) for all districts with at least five candidates. Then, I produced a histogram to summarize the distribution of the resulting SF ratios (Figure 1). Results for four- and five-seat districts (the other frequently occurring types of district in Japan) are given in Figures 2 and 3.

As can be seen, the SF distribution is bimodal in each case. Values near .5 are rare, relative to those near 1 or 0; that is, it is much more common to have either a close or a distant second loser than an "in-between" second loser. Moreover, the closer the first loser to the last winner, the more likely a few more votes might change the outcome, the further

FIGURE 1

Testing the Bimodality Hypothesis in 3-Seat Districts: Japanese Lower House Elections, 1958–90



Note: The SF ratio in any given district is found by dividing the second loser's vote total by the first loser's vote total.

from .5 is the SF ratio (i.e., the stronger is the tendency for the ratio to be either near 1 or near 0).

Are the distributions displayed in Figures 1–3 significantly bimodal? One can reject the null hypothesis that the distribution is unimodal in the first two cases, using dip or depth tests (see Hartigan and Hartigan 1985). In the third case, the probability of observing the degree of bimodality visible in Figure 3 is a bit below .2, under the null hypothesis that the distribution is really unimodal. Thus there is some reason to doubt that the distribution is unimodal, but the evidence is not conclusive at conventional levels of statistical significance. Using a kernel density-based test proposed by Silverman (1981), one can pit the null hypothesis of *bi*modality against the compound alternative of more than two modes. Doing so, one finds p-values of .22, .26, and .98 for the first, second, and third figures respectively. Thus one cannot reject the null of bimodality (in favor of multimodality) at conventional levels of significance. All told, the evidence is as it appears to be to the naked eye: the first two figures really are bimodal, and the third is harder to call but has some tendency toward bimodality.

Substantively, does all this mean that one can reject the null hypothesis of sincere voting? It all depends on what one believes the distribution of voters' true preferences over candidates is. If one believes that second losers' true support tends to be either almost equal to, or much less than, first losers' true support, then the evidence presented here is consistent with sincere voting. I can see no reason for such an expectation regarding the distribution of preferences, however, and accordingly take the statistical tendency toward bimodality to be evidence of strategic voting.

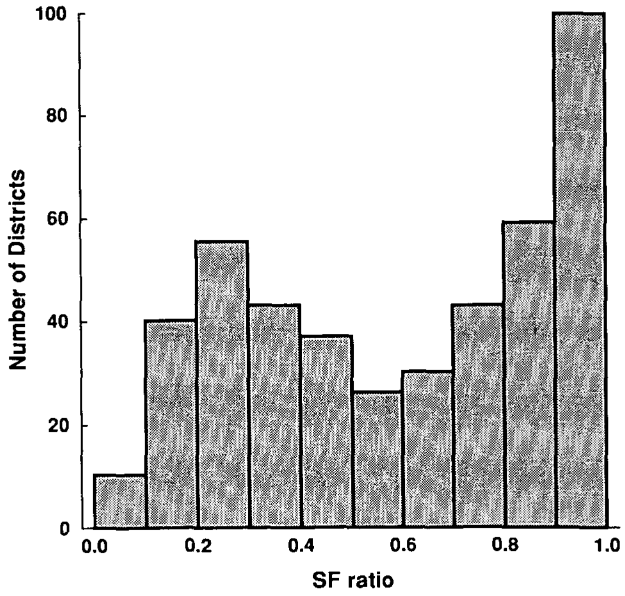
A second-order pattern that appears in the data is that the height of the mode near 0 declines with district magnitude. My interpretation of this is that the quality of voter information regarding candidate chances declines with district magnitude. In particular, it is harder to be sure who is trailing in a more crowded field in which very small vote percentages can win a seat. Whether this notion will hold up to scrutiny is a topic for further investigation.

Evidence that Japanese voters strategically desert leading candidates (those with more support than they need) is much less compelling than that they desert weak candidates. There is rarely anything like a dead heat among the top M candidates in a Japanese district, nor is there any movement toward such an ideal over time. Nonetheless, there is some evidence consistent with the model: (1) there is an over-time trend within the dominant Japanese party, the Liberal Democrats, toward fewer seats being lost on account of votes "wasted" on strong candidates (Cox and Niou 1994); (2) there is a statistically discernible tendency for fewer votes to be "wasted" on leading candidates when the margin of victory of the last winning candidate is narrower (indicating that votes switched from leading to marginal candidates are more likely to affect the outcome).

This last point is documented via an ordinary least squares regression, the results of which are reported in Table 1. The dependent variable, EXCESS, measures how far a particular district's race departs from the

FIGURE 2

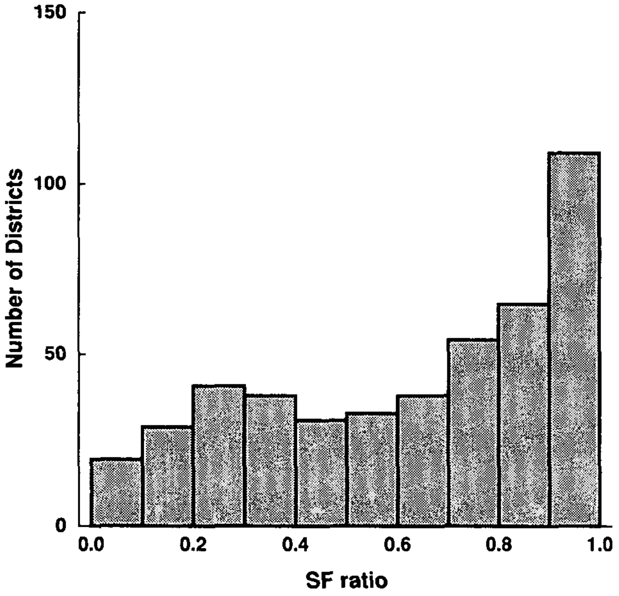
Testing the Bimodality Hypothesis in 4-Seat Districts: Japanese Lower House Elections, 1958–90



Note: The SF ratio in any given district is found by dividing the second loser's vote total by the first loser's vote total.

FIGURE 3

Testing the Bimodality Hypothesis in 5-Seat Districts: Japanese Lower House Elections, 1958–90



Note: The SF ratio in any given district is found by dividing the second loser's vote total by the first loser's vote total.

theoretical prediction that the vote percentages of the top *M* candidates will be the same. It is computed by calculating, for each of the top *M* − 1 finishers, the excess vote percentage they garner over that obtained by the *M*th-place finisher and then adding the resulting *M* − 1 figures. For example, in a three-seat district in which the top three candidates get 30%, 25%, and 20%, EXCESS = (30 − 20) + (25 − 30) = 15. The main independent variable in the analysis is MARGIN, equal to the vote percentage of the last winning candidate less that of the first losing candidate. The theoretical expectation is that smaller margins of victory will lead to fewer excess votes, since smaller margins indicate a better chance that votes currently wasted on leading candidates might affect the race between the marginal candidates, if transferred there.[10] Because five-seat districts have four candidates above the *M*th place while four-seat districts have three and three-seat districts only two, one expects more excess votes in larger-magnitude districts. It thus makes sense to control for district magnitude, which is done via two dummy variables.

Examining the results diplayed in Table 1, one finds a positive and statistically significant effect: a 1% reduction in the margin of victory of the last winner produces, on average, nearly a .5% reduction in votes wasted on leading candidates. There is thus a mild tendency for closeness in the race for the last seat in a district to reduce the number of votes wasted on strong candidates, even if there are still quite a few such wasted votes cast.

Alternative Explanations

Although the model of strategic voting generates empirically testable predictions, some of which are new (in the sense that they have not been noticed in the previous literature), there are also some obvious alternative explanations that might explain the pattern of evidence just uncovered. The problem is that *any* class of agents who care about the outcome of the election (not just voters but also activists, contributors, and candidates) will tend to allocate whatever resources they control (labor, money, etc.) to marginal candidates, where they are more likely to affect the outcome, rather than to trailing or leading candidates, where they are less likely to affect the outcome. Moreover, allocation or reallocation of resources to marginal candidates should produce the clearest aggregate results (trailing candidates deprived of all instrumental support, leading candidates of excess instrumental support) when who is marginal and who is nonmarginal is widely agreed and the margin of victory is small. Thus, the empirical evidence adduced is far from proving that a significant proportion of the electorate is instrumentally motivated and in reasonable agreement on the relative standing of the candidates. It may be that contributors give only (or mostly) to marginal candidates, that trailing candidates try to sell their endorsement to marginals, or that leading candidates sell some of their excess support to marginals.

The elite-level hypotheses have some distinct advantages. First, it is more plausible that elite actors, having larger stakes in the outcome, will pay attention to how close the race is and respond by diverting resources to marginal candidates. Put negatively, it is unlikely that ordinary voters will pay any attention at all, since their single votes have an infinitesimal chance of affecting the outcome. If it is at all costly to

TABLE 1

Closeness Depresses Excess Votes: Japanese Lower House Elections, 1958–90

	DEPENDENT VARIABLE: EXCESS	
INDEPENDENT VARIABLE	COEFFICIENT ESTIMATE	STANDARD ERROR
Constant	7.27	.35
Margin	.44	.04
Dummy for 4-seat districts	2.77	.42
Dummy for 5-seat districts	5.41	.43

Note: Adjusted $R^2 = .12$; N = 1,483. The variables are defined as follows. The unit of observation is an election in a particular district. Let V_j be the vote percentage garnered by the candidate placing *j*th in the poll in that district and let *M* be the district magnitude. Then (1) Excess = $\Sigma_{j=1}^{M-1} (V_j - V_M)$, (2) Margin = $V_M - V_{M+1}$, (3) Dummy for 4-seat districts = 1 if $M = 4$ (0 otherwise), and (4) Dummy for 5-seat districts = 1 if $M = 5$ (0 otherwise). As an example of the computation of Excess, consider a three-seat district in which the top three candidates receive 30%, 25%, and 20% of the vote. In this district, Excess = (30 − 20) + (25 − 20) = 15. The equation is estimated by ordinary least squares.

find out who is marginal or to calculate expected utilities, rational voters should avoid these costs, since bearing them has virtually no impact on the outcome (Meehl 1977; Riker 1982).

Second, the elite models look as if they can better accommodate the rather substantial amount of votes wasted on leading candidates. Contributors who seek "access" or specific postelection favors will stick with leading but not with trailing candidates. Leading candidates have something to lose if they sell off "too much" excess support, while trailing candidates, for all intents and purposes, do not.

Nonetheless, despite the apparent advantages of elite-based models, it is not clear that one can reject the voter-based model. The informational and cognitive costs of strategic voting are modest and may be borne entirely as byproducts of everyday activities, such as reading the newspaper, watching television, or attending college courses in politics. Information on the relative standings of candidates is sometimes published in polls; it does not take a rocket scientist to understand traditional wasted-vote arguments, and these arguments are sometimes hard to avoid (being urged by concerned elites). All this suggests that voters in the real world may strategically desert weak candidates for essentially the reasons stylized in the model. It is true that the whole process is mediated by elites: they point out that the race is close and that votes on weak candidates are wasted. But the voters do the rest: they buy the argument and act accordingly. Empirically, I think that there is overwhelming evidence that voters have, in a variety of historical and electoral contexts, voted strategically in this sense (see Cox 1991). The question of the relative importance of strategic reallocation of votes in the mass electorate as opposed to strategic reallocation of other resources in the elite strata remains open, however. And the evidence for strategic desertion of leading candidates is another story entirely. Elites in Japan do not seem to point out that votes for a sure winner are wasted, and there is no direct evidence at present that voters act as if they knew.

CONCLUSION

I have investigated strategic voting equilibria in multimember districts operating under SNTV, building on the previous work of Cox (1987), Palfrey (1989), and Myerson and Weber (1993). "Strategic voting" in single-member districts refers to a voter deserting a more preferred candidate with a poor chance of winning for a less preferred candidate with a better chance at winning. In multimember districts, voters who care only about the outcome of the election will strategically desert both candidates who are "too weak" and candidates who are "too strong." Such outcome-oriented voters desert weak candidates in multimember districts for the same reason as in single-member districts. They desert strong candidates when those candidates have one of the M seats sewn up but there are other seats still up for grabs; for then the voter's vote has a much greater chance of affecting the outcome if cast for one of the "marginal" candidates—those on the edge between winning and losing. All told, instrumentally motivated voters under SNTV waste their votes neither on weak (submarginal) nor on strong (supermarginal) candidates.

Equilibria of a "frictionless" model of strategic voting under SNTV—in which all voters are instrumentally motivated and all have rational expectations—are such that all wasted votes are wrung out of the system. As far as wasting votes on supermarginal candidates goes, this means that all winning candidates must be in a virtual dead heat in equilibrium, so that none will be perceived as having any "excess" votes that might profitably be transferred elsewhere. This prediction fares poorly in the Japanese data, although there is a tendency for fewer votes to be wasted on supermarginal candidates as the gap between last winner and first loser narrows.

Wringing out all votes wasted on submarginal candidates produces either Duvergerian outcomes (in which strategic voting erodes the support of all but one serious challenger, so that there are $M + 1$ viable candidates all told) or non-Duvergerian outcomes (in which two or more serious challengers are so closely matched that none becomes the unique victim of strategic desertion and more than $M + 1$ viable candidates survive). Empirically, outcomes with $M + 1$ viable candidates occur rather frequently. Thus, Duverger (1955) saw a tendency toward two-partyism in electoral systems employing single-member districts, while Reed (1991) saw a tendency for there to be four viable candidates in three-member districts, five viable candidates in four-member districts, and six viable candidates in five-member districts.

Both Duverger and Reed, of course, appealed to strategic voting (among other things) in explaining the observed tendency toward $M + 1$ viable candidacies (the $M + 1$ rule). The present model's utility is in illuminating some of the logical prerequisites and consequences of explaining the $M + 1$ rule in terms of strategic voting. As to prerequisites, the model shows that the degree to which strategic voting will winnow out weak candidates depends on how many instrumentally motivated voters there are and on how consistent their expectations about the relative standings of the candidates are. The empirical approximation of both these conditions plausibly depends on elite action and propaganda. American third-party movements (Ross Perot included) frequently emphasize *future* election outcomes: "We may have no real chance this time," they say, "but vote for us anyway, send a message, and help restructure American politics." The established party most hurt by the third party's appeals, in turn, is apt to emphasize the electoral here and now—the instrumental motivations highlighted in the present model. Similarly, elite actions determine how consistent voter beliefs are regarding who is winning and who is losing. If clear information about candidate chances is provided to voters, one can expect substantial strategic voting and a consequent reduction in the number of viable candidacies. If little (or conflicting) information is provided to voters, then greater amounts of sincere voting (or cross-cutting strategic voting) can be expected, and the tendency toward $M + 1$ viable candidates will be weaker.

As to the logical consequences of explaining the $M + 1$ rule in terms of strategic voting, the model provides specific and empirically testable predictions about what kind of exceptions to the $M + 1$ rule one should expect. Neither Duverger nor Reed do this. Both readily admit the possibility of exceptions to their generalizations: Duverger's Law and Reed's extension of it are both stated as "tendencies." But neither says much about the nature of the exceptions. Here, the theoretically allowable exceptions to the $M + 1$ rule have been characterized as various kinds of near ties, and a general implication of these exceptions (embodied in the bimodality hypothesis) has been tested with Japanese data.

There are of course other possible avenues to explore in explaining the $M + 1$ rule. Both Duverger and Reed appropriately suggest that elites may get into the act. Meehl (1977) and Riker (1982) argue that voters have too small a stake in elections to motivate strategic voting and emphasize elite actors even more strongly. Here I have noted that strategic reallocation of resources by outcome-oriented elite actors (activists, contributors, candidates) should produce many of the same aggregate patterns as identified in the voters-only model. My personal bias is strongly toward the elite-level hypotheses, as it is in the study of turnout (Cox and Munger 1989). I think strategic voting survives, both in theory and in practice, because one of the things outcome-oriented elites can do in close races to reallocate resources from nonmarginal to marginal candidates is to flood the mass media with wasted-vote arguments (including therein both the relevant evidence on candidate standings and the basic logic motivating a strategic vote). Finally, it should be noted that the basic model constructed here can be adapted to other electoral systems operating in multimember districts—in particular, to various forms of proportional representation. I intend to demonstrate this more fully in future work.

APPENDIX A: THE VOTER'S DECISION PROBLEM

I consider how a voter motivated solely by a desire to affect the outcome of the election decides whom to vote for, given that the voter votes. There are three parameters in the voter's decision (subscript is are suppressed): (1) the voter's preferences over the candidates, given by $u = (u_1, \ldots, u_K) \in U$; (2) the voter's expectations about how well each candidate will do at the polls, given by $\pi = (\pi_1, \ldots, \pi_K)$; and (3) the number of voters, n. I shall denote by $V(u; \pi, n) \subseteq K$ the optimal vote(s) of a voter of type u facing an electorate described by π and n. I shall show that the parameters identified are indeed sufficient to yield a well-defined decision problem and reveal such of the technical details of solving this problem as are necessary for proving the theorem to come.

Letting V_j denote the number of votes received by candidate j from voters other than the focal voter, the focal voter views $(V_1, \ldots, V_K)$ as K-nomial random variables with parameters π and $n - 1$. Thus, each voter's beliefs about the probabilities of various events (such as ties or near ties among the candidates) can be calculated using standard multinomial formulas. The details of this are given in Appendix B. Here, I shall simply assume that the probabilities of all events mentioned are known by the voter.

In particular, the focal voter knows, for all j and k: W_j, the probability that candidate j wins a seat outright (i.e., $|\{h \in K: V_h \geq V_j \text{ and } h \neq j\}| \leq M - 1$), and T_{jk}, the probability that candidates j and k are in a two-way tie for Mth place (i.e., $|\{h \in K: V_h > V_j\}| = M - 1$ and $|\{h \in K: V_h = V_j\}| = 2$). Following Hoffman (1982) and Myerson and Weber (1993), I assume that for large n, the perceived probability of an r-way tie for Mth, $r > 2$, is infinitesimal in comparison to the probability of a two-way tie for Mth. With this assumption, the focal voter's expected utility from abstaining, given u, π, and n, can be approximated with infinitesimal error by

$$\sum_{j=1}^{K} W_j u_j + \sum_{j=1}^{K} \sum_{k>j} T_{jk} \left(\frac{u_j + u_k}{2}\right),$$

where various terms involving r-way ties for Mth, $r > 2$, that are asymptotically negligible in comparison to the largest T_{jk}, have been omitted.

By voting for candidate j, the focal voter can affect his or her utility in two ways (continuing to ignore r-way ties, $r > 2$)—by putting j into a tie with k (yielding a utility increment of $(u_j + u_k)/2 - u_k$) or by breaking a tie between j and k (yielding a utility increment of $u_j - (u_j + u_k)/2$). Assuming (following Hoffman 1982 and Myerson and Weber 1993) that the probability of the event "k is in Mth place, tied with j" equals the probability of the event "k is in Mth place, one vote ahead of j," voting for candidate j rather than abstaining yields an expected utility increment of

$$\xi_j = \sum_{k=1}^{K} T_{jk}(u_j - u_k).$$

Thus,

$$V(u; \pi, n) = \arg\max_{j \in K} \xi_j.$$

APPENDIX B: MULTINOMIAL PROBABILITIES

I shall use the following notation throughout:

R = the set of real numbers;

Q = the set of rational numbers;
Z = the set of nonnegative integers;
ϕ = the standard normal density function;
$\boldsymbol{K} = \{1, \ldots, K\}$, the set of candidates;
$V_1, \ldots, V_K$ are K-nomially distributed random variables with parameters $\pi_1 \geq \pi_2 \geq \ldots \geq \pi_K$ and m;
T_{jk} = probability that candidates j and k are tied for Mth place, $0 < M < K$; and
$\Delta^{K-1} = \{(p_1, \ldots p_K) \in R^K: 0 \leq p_h \leq 1$ for all h & $\Sigma p_h = 1\}$, the simplex in K-space.

I shall explore the asymptotic properties of the tie-probabilities T_{jk}. These probabilities can be expressed as follows. For any set $S \subseteq \boldsymbol{K}$, $|S| = M - 1$, let $\text{up}_j(S)$ denote the event $\cap_{i \in S} V_i > V_j$, $\text{down}_j(S)$ denote the event $\cap_{i \in S} V_i < V_j$, and $E_{jk}(S) = \text{up}_j(S) \cap \text{down}_j(\boldsymbol{K} \backslash S \backslash \{j, k\}) \cap V_j = V_k$. In words, $E_{jk}(S)$ is the event "the $M - 1$ candidates in S finish ahead of j and k, j and k are tied for Mth, and all other candidates finish below j and k." With these definitions,

$$T_{jk} = \sum_{b=1}^{\binom{K-2}{M-1}} \Pr[E_{jk}(S_{jk}^b)],$$

where the sets S_{jk}^b all have $M - 1$ elements and correspond to the different ways of selecting which $M - 1$ candidates—from the $K - 2$ that remain after removing j and k—will have vote totals exceeding j's. The only one of these sets that it is necessary explicitly to define is

$$S_{jk}^1 = \begin{cases} \{1, \ldots, M-1\} & \text{if} \quad j \geq M \\ \{1, \ldots, M\} \backslash \{j\} & \text{if} \quad j < M < k \\ \{1, \ldots, M+1\} \backslash \{j, k\} & \text{if} \quad k \leq M. \end{cases}$$

This is the "natural" ordering of candidates other than j and k. The next lemma is useful in showing that $\Pr[E_{jk}(S_{jk}^1)] \geq Pr[E_{jk}(S_{jk}^b)]$ for all b.

LEMMA 1. *Suppose* $\mathrm{K} > \mathrm{M} + 1$ *and consider any* $\mathrm{A} \subset \mathbf{K} \backslash \{\mathrm{j, k}\}$ *such that* $|\mathrm{A}| = \mathrm{M} - 1$. *Denote the complement of* A *in* $\mathbf{K} \backslash \{\mathrm{j, k}\}$ *by* $\mathrm{A^c}$. *Suppose there exists* $\mathrm{d} \in \mathrm{A}$, $\mathrm{u} \in \mathrm{A^c}$, such that $\pi_\mathrm{u} > \pi_\mathrm{d}$, *and define* $\mathrm{B} = \mathrm{A} \backslash \{\mathrm{d}\} \cup \{\mathrm{u}\}$. *Then* $\Pr[\mathrm{E_{jk}(A)}] < \Pr[\mathrm{E_{jk}(B)}]$.

Proof. Note that

$$\Pr[E_{jk}(A)] = \sum_{v \in \bar{E}_{jk}(A)} P(v),$$

where

$$P(v) = m! \prod_{j=1}^{K} \frac{\pi_j^{v_j}}{v_j!}$$

and

$$\bar{E}_{jk}(A) = \left\{ v \in Z^K: \bigcap_{h \in K} V_h = v_h \in E_{jk}(A) \right\}.$$

Consider any term $v \in \bar{E}_{jk}(A)$. The vector v', defined by $v'_h = v_h$ for $h \notin \{d, u\}$, $v'_d = v_u$, and $v'_u = v_d$, is an element of $\bar{E}_{jk}(B)$. Moreover, $P(v)/P(v') = (\pi_d/\pi_u)^{V_d - V_u} \leq 1$ (recall that $\pi_d < \pi_u$ and $v_d > v_u$ since $v \in \bar{E}_{jk}(A)$).
QED.

COROLLARY 1. $Pr[\mathrm{E_{jk}(S_{jk}^1)}] \geq Pr[\mathrm{E_{jk}(S_{jk}^b)}]$ *for all* b.

I omit the proof: it follows directly from Lemma 1. In what follows, I shall abbreviate $E_{jk}(S_{jk}^1)$ by E_{jk}.

LEMMA 2. *Let* a, b, *and* c *be elements of* **K** *such that either (1)* $\mathrm{a} < \mathrm{M}$ *and* $\mathrm{a} < \mathrm{b} \leq \mathrm{M} + 1$ *and* $(\mathrm{b} = \mathrm{M} + 1 \rightarrow \mathrm{c} \leq \mathrm{M})$ *and* $\pi_\mathrm{a} > \pi_\mathrm{b}$ *or (2)* $\mathrm{a} > \mathrm{M} + 1$ *and* $\mathrm{M} \leq \mathrm{b} < \mathrm{a}$ *and* $(\mathrm{b} = \mathrm{M} \rightarrow \mathrm{c} \geq \mathrm{M})$ *and* $\pi_\mathrm{a} < \pi_\mathrm{b}$. *Then* $\lim_{\mathrm{m} \rightarrow \infty} \mathrm{T_{ca}/T_{cb}} = 0$.

Proof. By Corollary 1, it suffices to show that $\lim_{m \rightarrow \infty} \Pr[E_{ca}]/\Pr[E_{cb}] = 0$. The proof proceeds separately for the two cases. In case 1, note that $a < M$ implies $a \in S_{cb}^1$. Note also that $b \leq M$ implies $b \in S_{ca}^1$, since

$$S_{ca}^1 = \begin{cases} \{1, \ldots, M\} \backslash \{a\} & \text{if} \quad M < c \\ \{1, \ldots, M+1\} \backslash \{a, c\} & \text{if} \quad c \leq M. \end{cases}$$

When $b = M + 1$, moreover, $c \leq M$ by hypothesis, and hence $b \in S_{ca}^1$ again. In light of these observations, one can write $E_{ca} = E \cap (V_b > V_c = V_a)$ and $E_{cb} = E \cap (V_a > V_c = V_b)$, where $E = \text{up}_c(S_{ca}^1 \cap S_{cb}^1) \cap \text{down}_c(\boldsymbol{K} \backslash S_{ca}^1 \cap S_{cb}^1 \backslash \{a, b, c\})$.

Let $\pi_{h,a} = E(V_h/m|E_{ca})$ and $\pi_{h,b} = E(V_h/m|E_{cb})$. Note that $\pi_h > 0$ implies $\pi_{h,a} > 0$ and $\pi_{h,b} > 0$. Let $\sigma_{h,a}$ and $\sigma_{h,b}$ denote the conditional standard deviations. Note that they are both finite and that both approach zero as m approaches infinity. Thus the distribution of $(V_1/m, \ldots, V_K/m)$, conditional on E_{ca}, collapses around its mean; that is, for any $\varepsilon > 0$,

$$\lim_{m \rightarrow \infty} \Pr[\bigcap_{h=1}^{K} (\pi_{h,a} - \varepsilon < V_h/m < \pi_{h,a} + \varepsilon) | E_{ca}] = 1.$$

A similar statement can be made for the distribution of $(V_1/m, \ldots, V_K/m)$ conditional on E_{cb}.

Let S be the event

$$\bigcap_{h=1}^{K} \frac{V_h}{m} > \min \{\pi_{h,a}, \pi_{h,b}\} - \varepsilon.$$

Note that because

$$S \supseteq \bigcap_{h=1}^{K} (\pi_{h,a} - \varepsilon < V_h/m < \pi_{h,a} + \varepsilon),$$

$$\lim_{m \rightarrow \infty} \Pr[S | E_{ca}] = 1.$$

Similarly,

$$\lim_{m \rightarrow \infty} \Pr[S | E_{cb}] = 1.$$

In light of these results,

$$\lim_{m \rightarrow \infty} \frac{\Pr[E_{ca} \cap S^c]}{\Pr[E_{ca} \cap S]} = \lim_{m \rightarrow \infty} \frac{\Pr[E_{ca}](1 - \Pr[S | E_{ca}])}{\Pr[E_{ca}]\Pr[S | E_{ca}]} = 0.$$

Thus since

$$\frac{\Pr[E_{ca}]}{\Pr[E_{cb}]} = \frac{\Pr[E_{ca} \cap S] + \Pr[E_{ca} \cap S^c]}{\Pr[E_{cb} \cap S] + \Pr[E_{cb} \cap S^c]},$$

it suffices to show that

$$\lim_{m \to \infty} \frac{\Pr[E_{ca} \cap S]}{\Pr[E_{cb} \cap S]} = 0.$$

But

$$\frac{\Pr[E_{ca} \cap S]}{\Pr[E_{cb} \cap S]} = \frac{\Pr[E \mid S] \sum_{p \in \Xi} f_m(p)}{\Pr[E \mid S] \sum_{p \in \Xi} g_m(p)},$$

where

$$f_m(p) = \Pr[\bigcap_{h \in W} V_h = p_h m \mid E \cap S]\Pr[V_b > p_c m$$

$$= V_a \mid \bigcap_{h \in W} V_h = p_h m],$$

$$g_m(p) = \Pr[\bigcap_{h \in W} V_h = p_h m \mid E \cap S]\Pr[V_a > p_c m$$

$$= V_b \mid \bigcap_{h \in W} V_h = p_h m],$$

$$\Xi = \{p \in \Delta^{K-1} \cap Q^K\colon \bigcap_{h \in W} V_h$$

$$= p_h m \subseteq E \cap S \text{ for some } m \in Z\} \text{ and}$$

$W = \boldsymbol{K} \backslash \{a, b\}$.

Fix $p \in \Xi$ and consider the limit as m approaches infinity of $f_m(p)/g_m(p)$. Note that

$$\frac{f_m(p)}{g_m(p)} =$$

$$\begin{cases} 0/0 & \text{if} \quad p \notin \Xi_m \\ \dfrac{\Pr[V_b > p_c m = V_a \mid \bigcap_{h \in W} V_h = p_h m]}{\Pr[V_a > p_c m = V_b \mid \bigcap_{h \in W} V_h = p_h m]} & \text{if} \quad p \in \Xi_m, \end{cases}$$

where $\Xi_m = \{p \in \Xi\colon p_h m \in Z \text{ for all } h \in W\}$. Note also that

$$\lim_{\substack{m \to \infty \\ m \in \Xi^{-1}(p)}} \frac{f_m(p)}{g_m(p)} = 0,$$

where

$$\Xi^{-1}(p) = \{m \in Z\colon p \in \Xi_m\}.$$

This last result is derived as follows. Let $v_h = p_h m$ for all h. Conditional on $\cap_{h \in W} V_h = v_h$, V_a and V_b are distributed binomially, with parameters $\pi_a^* = \pi_a/(\pi_a + \pi_b)$, $\pi_b^* = \pi_b/(\pi_b + \pi_a)$, and $m^* = m - \Sigma_{h \in W}\, v_h$. If $2v_c \geq m^*$, then both probabilities are zero. Conditional on $2v_c < m^*$, we have $(V_a = v_c \rightarrow V_b > v_c)$ and $(V_b = v_c \rightarrow V_a > v_c)$, since $V_a + V_b = m^* > 2v_c$. Thus (the condition $m \in \Xi^{-1}(p)$ being understood in all that follows)

$$\lim_{m \to \infty} \frac{f_m(p)}{g_m(p)} = \lim_{m \to \infty} \frac{\Pr[v_c = V_a \mid \bigcap_{h \in W} V_h = v_h]}{\Pr[v_c = V_b \mid \bigcap_{h \in W} V_h = v_h]}$$

$$= \lim_{m \to \infty} \frac{\phi(v_c - m^*\pi_a^*/\pi_a^*(1 - \pi_a^*))}{\phi(v_c - m^*\pi_b^*/\pi_b^*(1 - \pi_b^*))}$$

$$= \lim_{m \to \infty} \exp\left\{\frac{(v_c - m^*\pi_b^*)^2 - (v_c - m^*\pi_a^*)^2}{\pi_a^*(1 - \pi_a^*)}\right\}.$$

The last limit equals 0 because

$$\lim_{m \to \infty} v_c^2 - 2v_c m^* \pi_b^* + (m^*\pi_b^*)^2 - (v_c^2 - 2v_c m^* \pi_a^*$$

$$+ (m^*\pi_a^*)^2) = \lim_{m \to \infty} (m^*)^2(\pi_a^* - \pi_b^*)\left(\frac{2v_c}{m^*} - 1\right) = -\infty.$$

The last equality follows because $m^* > 2v_c = 2p_c m$ and $p_c > 0$. Thus, $\lim m^* = \infty$, while the second term is a positive constant and the third term a negative constant.

In case 2, $a > M + 1$ implies $a \notin S_{cb}^1$. Moreover, since

$$S_{ca}^1 = \begin{cases} \{1, \ldots, M - 1\} & \text{if} \quad M \leq c \\ \{1, \ldots, M\} \backslash \{c\} & \text{if} \quad c < M, \end{cases}$$

$b \geq M + 1$ and $b = M$ (since this entails $c \geq M$) both imply $b \notin S_{ca}^1$. In light of these observations, one can write $E_{ca} = E \cap (V_c = V_a > V_b)$ and $E_{cb} = E \cap (V_c = V_b > V_a)$, where $E = \text{up}_c(S_{ca}^1 \cap S_{cb}^1) \cap \text{down}_c(\boldsymbol{K} \backslash S_{ca}^1 \cap S_{cb}^1 \backslash \{a, b, c\})$.

Given these definitions, the proof here proceeds identically to that in the first case if one substitutes the string '$p_c m = V_a > V_b$' for every occurrence of the string '$V_b > p_c m = V_a$' and the string '$p_c m = V_b > V_a$' for every occurrence of the string '$V_a > p_c m = V_b$'. Somewhat larger changes are needed when one examines the limit of the ratio $f_m(p)/g_m(p)$, where, in this case,

$$\frac{f_m(p)}{g_m(p)} =$$

$$\begin{cases} 0/0 & \text{if} \quad p \notin \Xi_m \\ \dfrac{\Pr[p_c m = V_a > V_b \mid \lim_{h \in W} V_h = p_h m]}{\Pr[p_c m = V_b > V_a \mid \lim_{h \in W} V_h = p_h m]} & \text{if} \quad p \in \Xi_m. \end{cases}$$

Conditional on $\cap_{h \in W} V_h = v_h$, V_a and V_b are distributed binomially, with parameters $\pi_a^* = \pi_a/(\pi_a + \pi_b)$, $\pi_b^* = \pi_b/(\pi_b + \pi_a)$, and $m^* = m - \Sigma_{h \in W} V_h$. If $m^* < v_c$ or $m^* \geq 2v_c$, then both probabilities are zero. Conditional on $v_c \leq m^* < 2v_c$, we have ($V_a = v_c \rightarrow V_a > V_b$) and ($V_b = v_c \rightarrow V_b > V_a$). Thus (the condition $m \in \Xi^{-1}(p)$ being understood in all that follows),

$$\lim_{m \to \infty} \frac{f_m(p)}{g_m(p)} = \lim_{m \to \infty} \frac{\Pr[v_c = V_a | \lim_{h \in W} V_h = v_h]}{\Pr[v_c = V_b | \lim_{h \in W} V_h = v_h]}$$

$$= \lim_{m \to \infty} \frac{\phi(v_c - m^* \pi_a^* / \pi_a^*(1 - \pi_a^*))}{\phi(v_c - m^* \pi_b^* / \pi_b^*(1 - \pi_b^*))}$$

$$= \lim_{m \to \infty} \exp\left\{ \frac{(v_c - m^* \pi_b^*)^2 - (v_c - m^* \pi_a^*)^2}{\pi_a^*(1 - \pi_a^*)} \right\}.$$

The last limit equals zero because

$$\lim_{m \to \infty} v_c^2 - 2v_c m^* \pi_b^* + (m^* \pi_b^*)^2 - (v_c^2 - 2v_c m^* \pi_a^*$$

$$+ (m^* \pi_a^*)^2) = \lim_{m \to \infty} (m^*)^2 (\pi_a^* - \pi_b^*)(2v_c m^* - 1) = -\infty.$$

The last equality follows because $m^* < 2v_c$ (so the last term is a positive constant), the second term is a negative constant, and $m^* \geq p_c m > 0$ (so $\lim m^* = \infty$). QED.

LEMMA 3. *(a) If* $\pi_1 > \pi_M$, *then* $\lim_{m \to \infty} T_{1j} / T_{M,M+1} = 0$ *for all* j. *(b) If* $0 < \pi_j < \pi_{M+1}$ *for some* $j > M + 1$, *then* $\lim_{m \to \infty} T_{jk} / T_{M,M+1} = 0$ *for all* k.

Proof Part 1. By Corollary 1, it suffices to show that $\lim_{m \to \infty} \Pr[E_{1j}]/\Pr[E_{M,M+1}] = 0$ for all j. And this follows because (1) $\lim_{m \to \infty} \Pr[E_{1j}]/\Pr[E_{1M}] = 0$ for all $j < M$ (by Lemma 2, case 1); (2) $\lim_{m \to \infty} \Pr[E_{1j}]/\Pr[E_{1,M+1}] = 0$ for all $j > M + 1$ (by Lemma 2, case 2); (3) $\lim_{m \to \infty} \Pr[E_{1M}]/\Pr[E_{M,M+1}] = 0$ (by Lemma 2, case 1); and (4) $\lim_{m \to \infty} \Pr[E_{1,M+1}]/\Pr[E_{M,M+1}] = 0$ (by Lemma 2, case 1).

Proof Part 2. By Corollary 1, it suffices to show that $\lim_{m \to \infty} \Pr[E_{jk}]/\Pr[E_{M,M+1}] = 0$ for all k. And this follows because (1) $\lim_{m \to \infty} \Pr[E_{jk}]/\Pr[E_{j,M+1}] = 0$ for all $k > M + 1$ (by Lemma 2, case 2); (2) $\lim_{m \to \infty} \Pr[E_{jk}]/\Pr[E_{jM}] = 0$ for all $k < M$ (by Lemma 2, case 1); (3) $\lim_{m \to \infty} \Pr[E_{j,M+1}]/\Pr[E_{M,M+1}] = 0$ (by Lemma 2, case 2); and (4) $\lim_{m \to \infty} \Pr[E_{jM}]/\Pr[E_{M,M+1}] = 0$ (by Lemma 2, case 2). QED.

Proof of Theorem 1. Suppose $\pi_1 > \pi_M$ and consider a voter of type u. Let $X_j = \{k \in K: \pi_k = \pi_j\}$ and call any candidate in $X_M \cup X_{M+1}$ "marginal." Let h be the marginal candidate whom the voter ranks highest: $u_h > u_j$ for all $j \in X_M \cup X_{M+1} \backslash \{h\}$. (Such a candidate exists because all voters have strict preferences among candidates.)

Now consider ξ_h^n, the expected utility increment of voting for h rather than abstaining, when the electorate is of size n. From Appendix A, $\xi_h^n = \Sigma_{k=1}^K T_{hk}^n (u_h - u_k)$, where T_{hk}^n *is the probability of a two-way tie for* M*th between* h and k, given an electorate of size n (and given π). Note that $\xi_h^n > 0$ for all n, because (1) in the limit, ties between marginal candidates dominate those involving nonmarginal candidates, by Lemma 2 and (2) $u_h - u_k > 0$ for all $k \in X_M \cup X_{M+1} \backslash \{h\}$. Note also that $\lim \xi_1^n / \xi_h^n = 0$, by Lemma 3. (One can assume without loss of generality that $h \leq M + 1$, since $h \in X_M \cup X_{M+1}$.) Thus, in the limit, no voter will vote for candidate 1, given expectations π, which contradicts the expectation that $\pi_1 > 0$. This shows that for large enough n, π is not a rational expectation. But there may be rational expectations arbitrarily close to π.

In order to show that one cannot find any rational expectations "close" to π, consider a sequence $\{\pi^n\}$ of rational expectations that converges to π. The discussion leading up to Lemma 1, and the proof of that lemma, show that

$$T_{hk}^n = \sum_{b=1}^{\binom{M-1}{K-2}} \sum_{v \in E_{hk}(S_{hk}^b)} (n-1)! \prod_{j=1}^{K} \frac{\pi_j^{v_j}}{v_j!},$$

where

$$\bar{E}_{hk}(S_{hk}^b) = \{v \in Z^K: \lim_{h \in K} V_h = v_h \in E_{hk}(S_{hk}^b)\}.$$

Thus T_{hk}^n is a continuous function of π, and so is ξ_h^n. Let $\xi_h^n(\pi^n)$ be the expression resulting when one substitutes π^n for π in the expression for ξ_h^n. The continuity of ξ_h^n in π, together with $\pi^n \rightarrow \pi$ and $\xi_h^n > 0$ for all n, implies $\xi_h^n(\pi^n) > 0$ for all n sufficiently large. The ratio ξ_1^n/ξ_h^n is continuous in π, over the set of all π such that ξ_h^n is not zero. Thus $\lim \xi_1^n(\pi^n)/\xi_h^n(\pi^n) = \lim \xi_1^n/\xi_h^n = 0$, the latter equality having been established above; that is, in sufficiently large electorates with expectations sufficiently close to π, no voter will vote for candidate 1, contrary to the assumption that $\pi_1 > 0$. A similar proof works for the other part of the theorem. QED.

Notes

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1. The Japanese and Taiwanese require that a candidate get above a legally defined minimum vote share in order to win a seat. The seat allocation rule, in other words, is not pure plurality. Nonetheless, this provision is of negligible practical importance.

2. The adjective *strategic* in *strategic voting equilibria* is really superfluous in that agents in the model conform to the usual postulate of rationality and are not distinctly more strategic than agents in any other rational choice model. The term *strategic voting* is well established in the literature, however, and I shall follow conventional usage.

3. Myerson and Weber (1993) suggest that considerations of dynamic consistency or persistence may eliminate the non-Duvergerian equilibria but do not demonstrate this.

4. Myerson and Weber (1993) show that neither one of these assumptions is crucial.

5. Formally, the assumption is this. Let each voter order the candidates from largest π_{ij} to smallest. Let $M(i)$ be the candidate ranked Mth in i's ordering and $M + 1(i)$ be the candidate ranked $(M + 1)$st. Let $\mathrm{Mid}(i) = \{j\colon \pi_{ij} = \pi_{iM(i)}$ or $\pi_{ij} = \pi_{iM+1(i)}\}$, $\mathrm{Lead}(i) = \{j\colon \pi_{ij} > \pi_{iM(i)}\}$, and $\mathrm{Trail}(i) = \boldsymbol{K}\backslash\mathrm{Mid}(i)\backslash\mathrm{Lead}(i)$. Then, for all voters i and h, $\mathrm{Lead}(i) = \mathrm{Lead}(h)$ and $\mathrm{Trail}(i) = \mathrm{Trail}(h)$.

6. Because there are no atoms in the type distribution F, nonsingleton $V(u; \pi, n)$s do not require special handling.

7. I thank Roger Myerson and Barry Nalebuff for greatly clarifying what this theorem ought to say.

8. Trailing candidates find allies in their attempts to avoid the logic of the wasted vote in front-runners who expect a net loss should the trailing candidacy go down the tubes (e.g., recall Ronald Reagan's support of John Anderson's candidacy in 1980) and foes in front-runners who expect a net gain of support (recall Jimmy Carter's persistent reminders to voters not to waste their vote on Anderson).

9. The data used in this analysis are from Steven Reed's compendium *Japan Election Data: The House of Representatives 1947–1990* (Ann Arbor: Center for Japanese Studies, 1992).

10. Note that there is an artifactual expectation of precisely the opposite character. Looking at the mathematical definitions of EXCESS and MARGIN given in the notes to Table 1, one sees that V_M, the vote percentage of the Mth-place candidate, appears with a negative sign in the expression for EXCESS and with a positive sign in the expression for MARGIN. Thus one expects an artifactual negative relationship. If the theoretically expected positive relationship nonetheless appears, one can be more confident that it corresponds to some real political effect.

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THE TOWER OF BABEL AS A COORDINATION GAME: POLITICAL LINGUISTICS IN GHANA

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The problem of choosing an indigenous official language for multilingual states in general and for Ghana in particular is treated as one of coordination in an n-person tipping game. Even with an assumption that the equilibrium outcome of all-English is deficient, the mechanisms for reaching an indigenous language solution are difficult to find. A lottery (a theoretically attractive approach to the solution of coordination games) is less attractive when applied to the issue of language coordination. Empirical data based on interviews from six different regions of Ghana show the limits and possibilities of the theoretic solution. Going back to theory, a mechanism for the successful promotion of an indigenous language outcome is proposed.

God's punishment for the audacity of building the Tower of Babel sets up a neat n-person coordination game for the world's scattered peoples. Individuals from each language group may want to communicate directly with members from other language groups, but they have to agree upon a common language to do so.[1]

In world history, the Babelian curse has been negotiated in a variety of ways. The most common solution is based on simple market conditions: high levels of contact between peoples of different language groups induce new speech forms, permitting intergroup communication. Over a long expanse (e.g., of a river or through a subcontinent), linguists have discovered language chains. People at point A communicate effectively but haltingly with neighbors at point B, who themselves communicate with their neighbors at point C, and so on; but people at point A cannot communicate at all with people at point N. Dialect chains are not the only solution; often pidgins or lingua francas help overcome the Babelian curse.[2] All these solutions are based on the microdynamics of language coordination across space and time, without state-enforced contracts or legislation.

Market solutions are not always efficient. Consider two separated language groups that due to a technological change (say, the spanning of a river by a bridge) form a unified economic region. Suppose all people in this region are monolinguals in one of two languages, L and L1. It may be Pareto efficient (especially if transfer payments are permitted) for speakers of L to switch to L1, or vice versa. Even if it were agreed which language was a better coordination point, however, it might require a critical mass of people to change before it would be rational for any single person to change. This is the core insight in Schelling's "tipping game" (1978, chap. 7). Without any stimulus for organizing a critical mass of L or L1 speakers to switch, a class of professionals (whose learning costs are lower than the average for the population) will likely emerge and charge for translation services. Over a few generations this will be a costlier solution than early agreement by all on a common language.

Rulers have an interest in promoting efficient language outcomes. The failure of the market to overcome the inefficiencies of the Babelian curse adds to the transaction costs of rule, if only because governmental monitoring of compliance to its laws is more costly if the rules themselves must be translated into a number of languages. It adds further to the costs of rule if agents of the ruler need to translate local documents such as account books (for purposes of taxation) into the language of rule. Rulers interfere in the marketplace of languages not only because the transactions costs of rule increase when the language coordination problem is unsolved but also because lowering the transactions costs of business internal to the state raises the taxable surplus (Friedman 1977).[3]

This helps explain why numerous rulers of states and empires have sought to manipulate language use in the societies they rule. Rationalization—the authoritative imposition of a single language for educational and administrative communications—is only one possible strategy (Laitin 1988). Other forms of managed coordination, such as language zoning in Switzerland and an administratively and educationally induced lingua franca in the former Soviet Union, are state-influenced coordination outcomes.

Rulers have an interest not only in lowering transaction costs within their boundaries but in raising those costs across boundaries. Solving the Tower of Babel curse within a boundary may amplify that curse internationally. Despite the apparent efficiency of a single-world language, however, it may not be a superior equilibrium to one in which each country (or region) specializes in a single language. Taking cues from work on the optimal size of firms (Williamson 1986, chap. 3) and on the potential efficiencies of the development of particularistic corporate cultures (Kreps 1990), I give more weight here to the issue of creating national cultures and less to the issue of an efficient world-language regime.

Rulers of states that experienced colonial rule and received their independence in the twentieth century have faced a far more difficult task in rationalizing language within their boundaries than leaders who consolidated states in earlier centuries. These twentieth-century leaders of new states have found that language issues are particularly volatile when they become politicized. I shall explain why coordination on an indigenous national language has been difficult for such leaders. I will focus on the language situation in Ghana, but the dynamics of the situation are similar for many multilingual African countries that lack any indigenous lingua franca that could serve as common language of education, administration, and public life (e.g., Zaire, Nigeria, Senegal, and Ivory Coast).[4]

My goals here go beyond a strategic analysis of why state-induced coordination on a national language is difficult to achieve. Through a detailed case study of Ghana's linguistic history and a survey of language use and attitudes, I shall illustrate the difficulty of applying in the real world certain theoretically elegant solutions to the Tower of Babel curse. Finally, I shall propose a linguistic strategy that, while not yielding language rationalization, does indeed permit the institutionalization of an indigenous national language in countries like Ghana.

GHANA: SOCIOLINGUISTIC BACKGROUND

Ghana in the 1950s was the African country with the strongest and most publicized national movement. Kwame Nkrumah, the charismatic leader of the Convention Peoples party and of Ghana itself, was a committed pan-Africanist. He believed that true freedom demanded that the roots of European domination be dug out. Yet he, like virtually all African nationalists, had to rely on a European language to make his message understood by at least some persons in each part of his country. His first language was Nzema, a language spoken by only a small percentage of the people of Gold Coast (the colonial appellation for today's Ghana). What sort of language policy was possible under conditions that Nkrumah faced? Ghana, like nearly all African countries, seems to have been a principal victim of God's wrath aimed against those who constructed the Tower of Babel.

Ghana had a population of 14.5 million in 1980. The official language (i.e., the language used in educational institutions and for conducting government business) is English. Akan speakers make up 44.1% of the population. (Twi, Fanti, Brong, Akim, and Nzema, are the principal languages of the Akan family.) The Mole-Dagbani of the North and Upper regions constitute 16%, the Ewe of Volta Region 13%, Ga-Adangbe of Greater Accra and parts of the Eastern Region 8.3%, the Guang of the East and parts of the North 3.7%, the Gurma of the North and Upper regions 3.5%, and the remaining groups (including the Kasem in the North) 11.4% (Chazan 1983, 35). Very few Ghanaians consider themselves to be Hausas; yet the Hausa language is broadly understood throughout the northern regions, as well as across the west African savanna; it is probably the most widely understood language in west Africa today.

Linguists point to the broad zone of Akan languages in Ghana's southern and middle belts—an area in which there have historically been significant political, social, and economic interactions among peoples. There was, at least during the period of Asante dominance in the precolonial era, reasonable mutual intelligibility among speakers of these Akan languages. But missionary and colonial policies (at least up through World War II), by providing educational and administrative benefits based on tribal boundaries, gave incentives for local chiefs to emphasize linguistic differences from their neighbors. Hence, from the late nineteenth century, a spiral of language differentiation ensued. By 1950, Akan speakers were more differentiated linguistically than they had been 100 years earlier (Dickens 1953).

The colonial state's language policy was not fully coherent. On the one hand, and consistent with the Colonial Office's view that rule could be cheaply administered if colonial administrators ruled indirectly through native chiefs, the colonial state sought to encourage basic education in mother tongues. This is why it gave in to demands of local chiefs for the official recognition of their languages. On the other hand, ease of administration demanded that a class of Ghanaians be literate in English. Thus, a mixed policy developed. In the primary schools, the vernaculars usually served as the media of instruction; but in the secondary schools, the medium became English (Kitchen 1962, 334–36).

The independence movement sought both to overcome the reliance on foreign cultures and to induce a more unified African or Ghanaian culture. A search for a Ghanaian national language was thought to be a sure remedy. But the independent state could find no more coherent language policy than that of the colonial government. Whiteley nicely sums up the problem:

> In 1961 a Private Member's motion was tabled in the Ghana National Assembly to the effect that ". . . in the opinion of this House it is desirable that the Akan language should be taught in all schools in Ghana." . . . The motion was defeated, but during the debate many points were raised which have appeared again and again . . . whenever language matters are discussed. A majority were in favor of retaining English as the official language not only because of its international status, but also because of its unifying role within Ghana. Yet, even among those who supported its retention it was recognized that original contributions in, say, literature, might not be forthcoming so readily in English. . . . The Deputy Minister of Education, Mrs. Susannah Al-Hassan is reported to have summarized official policy in the following terms: ". . . it is the intention of Government to encourage the development of all our major national languages so that each of them may have an equal opportunity of attaining a standard which would improve its chances of being chosen as the national language when the time comes for such a decision. . . . In the meantime, I think honourable members will agree that we have enough on our plate to keep us busy for a few more years, without having to worry about a common language." (1971, 552–53).

Governments changed repeatedly over the next generation, but the language policy of Al-Hassan remained untouched. The staff at the Bureau of Ghana Languages in Accra has analyzed and developed a number of indigenous languages, but their books are usually neglected by school authorities, who often refuse to purchase or to assign them (*People's Daily Graphic* 1990).

In the early 1990s there was some movement on the official language front. Jerry Rawlings, Ghana's leader, inadvertently reactivated the language issue. In an attempt to reduce the power of Ghana's regions, Rawlings enhanced the powers of the districts and increased their number from 65 to 110. As a consequence of this change, nearly all districts are now linguistically homogeneous. In the first elections for district assemblies, candidates who had no facility in English were permitted to run. Political debate in the assemblies, especially in rural districts, began to take place in indigenous languages. Debates have been lively and participation active. This is the most important official use of indigenous languages in modern Ghana even if English remains the sole official language of law and administration.[5]

The vibrancy of political discourse relying on indigenous languages in the district assemblies reflects the sociolinguistic reality in which the indigenous languages of Ghana are used in many walks of life. Even in the schools, the indigenous languages have a semiofficial role. In a 1991 survey of Cape Coast, where English is extremely prestigious and widely used, Fanti remains a language of considerable importance in many social domains.[6] To be sure, English was the official medium of instruction throughout primary, junior secondary, and senior secondary schools; yet in 95% of the primary schools surveyed, teachers reported using Fanti as a medium of instruction instead of English, at least some of the time. Most teachers reported that the children who begin school cannot understand basic directions or concepts in English. At junior secondary school, a few teachers report using Fanti to explain concepts, but English becomes the language of instruction and continues that way up through the grades. English dominates recreational reading at all levels largely because the only available works in Fanti are specialized language and literature texts.

But Fanti does not progressively get eliminated from the lives of advanced students. Through secondary school, Fanti predominates over English on the soccer fields and in informal singing. Furthermore, Fanti is a required subject for students all through their school careers. And teachers report almost unanimously that non-Fanti students have little trouble in learning Fanti. In our survey, no teacher reported any resistance of non-Fanti students in learning Fanti, and many reported that non-Fanti students often did better in the Fanti examinations than the Fanti students. Fanti remains the language of informal domains. Speaking English in those domains is considered overly formal and distant.

Pidgin has specialized functions in Cape Coast schools—to tell off-color jokes or to relate incidents in highly informal settings that are especially amusing.[7] But teachers are so emphatic that they never—and their students rarely—use pidgin that one suspects otherwise. Many teachers put exclamation points after the *no* in answer to our question as to whether they use pidgin in the staff room. One teacher wrote that pidgin "is used by students when they want to express themselves without others understanding them." This comment reveals more about the teacher's values than it does about the use of pidgin. The project supervisor, however, observed interactions in these schools and claimed that there was no underreporting of pidgin. Results from a wider survey (to be described shortly) come to a similar result. When asked what they would do if a young person addressed them in pidgin, only 15% of our respondents said they would answer in pidgin. On the question of whether Ghanaian writing should rely on the Queen's English, a large number of respondents felt it important to keep to the international standard. "If we are learning someone's language," one respondent offered, "then it is better we learn it in its original form so that he will not have any advantage over us" (K11).[8]

If pidgin remains limited in use and low in prestige in Ghana today, there is a speech form that relies heavily on Akan–English code switching, and this form is evidently used far more widely than even its speakers recognize.[9] In interviews, a number of speakers denied relying on it while using it extensively in those very interviews. The speech form plays an important role in interethnic communication and has no overtones of belonging to a particular group, despite its extensive reliance on Akan languages. Furthermore, linguistic analysis of this speech form suggests its emergence as a natural language (Forson 1979). I shall refer to this emergent language as *Ghanaian*. In contemporary Ghana, Ghanaian has a role not unlike that of "English" during the period of Falstaff and Prince Hal: it is a widely spoken language of popular culture whose speakers do not consider it a "real" language.

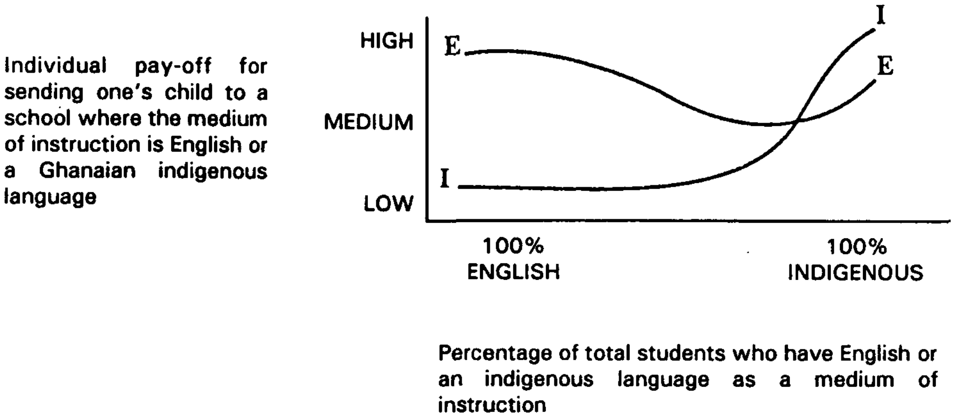
In contemporary Ghana, African languages remain in limbo. English is clearly the language of expertise, of central administration, of international business. Yet the African languages remain vibrant in many social domains, and since there are virtually no monolingual Ghanaians in English, there are no discernible trends pointing to the death of African languages. Nationalism seems strong because Ghanaians retain their ancestral languages with low economic pay-offs for doing so; yet it seems weak because Ghanaians put up little resistance to the ever-wider use of English in public life.

LANGUAGE COORDINATION AND THE TIPPING GAME

The "story" that motivates my theoretical models is as follows. Most Ghanaian citizens are convinced that

FIGURE 1

The Language Tipping Game in Ghana



it is necessary to learn English to get a well-paid job in the modern sector and surmise from this that it is unrealistic to educate their children through the medium of an indigenous language. But most Ghanaians have inchoate feelings that continued reliance on English is tearing them from their own cultural roots. This reliance is seen as a remnant of colonialism. In terms of linguistic policy, Ghanaians are pulled in opposite directions.

This ambivalence can be portrayed graphically (see Figure 1) as a *tipping game* (Schelling 1978, chap. 7).[10] The Babelian tipping game has six characteristics. First, the binary choice is for parents to enroll their children in English medium schools (where indigenous languages become restricted for family domains) or to seek out schools where a Ghanaian language is the medium of instruction (and where English or other international languages are taught only as subjects).

Second, virtually all schools in Ghana rely on English as the medium of instruction, and all jobs in the modern sector require a good working knowledge of English. The payoffs for educating your child in English when 100% of official business is conducted in English are far higher than the payoffs for educating your child in an indigenous language. This represents the present equilibrium.

Third, the game is one of coordination, not pure conflict. Each Ghanaian parent wants her children to be educated in the common language of official business and elite discourse. An outcome in which Ghanaians used languages that were not easily understood by large numbers of fellow Ghanaians for correspondence, petitions, and television programs would bring substantial disutility to all. (But at 100%-English, any mover to the indigenous language would suffer great losses, while the loss for n − 1 Ghanaians would be small.)

Fourth, figure 1 indicates that if there were coordination in an indigenous language (i.e., 0% of official business conducted in English), the individual payoff for having been educated in that indigenous language would be higher than the payoff for having learned English when 100% of official business is conducted in English. (This assumption will be given a plausibility test in the empirical data to be presented shortly.)

Fifth, calculating the value to an individual at 100%-indigenous is complicated by the fact that individuals do not know which indigenous language would be official. To calculate their value for education in an indigenous language, then, individuals must rely on calculations of expected utility. Suppose we were to ask an individual *i* about personal utility for an outcome that is at 100%-indigenous in a country with *k* languages. Individual *i* would sum the probability of each language in set *k* being designated as the official language multiplied by personal utility for that language at 100%-indigenous. Her calculation would be:[11]

$$EU_i = \sum_{\forall k} P_k U_{ik}$$

Since the expected utility of individuals from different language groups differs, line *I–I* on Figure 1 represents the average payoff for all citizens, whose individual payoffs may diverge considerably from the mean. (The wide divergence, based on the different utilities for speakers of different languages, makes the Ghana case different from representations of regional revival movements—in Quebec, Flanders, and Catalonia for example—where there has been agreement about the language that would replace the status quo.)

Sixth, this tipping model captures two coordination problems. First, we have a game of pure coordination. We see that 100%-indigenous may be the Pareto superior equilibrium (i.e., at least as good as the other equilibrium for all and better than the other for at least one), but it would be irrational for anyone to move unilaterally from the status quo. This is because the status quo is a Nash equilibrium, which means that no player has an incentive to leave unless coordinated with other players.

The second coordination problem resembles the game *battle of the sexes*. (In this game, both members of a couple prefer to be together but differ as to where it should be. Each would rather prevail but both would rather coordinate than separate. The problem is where to go.) The coordination problem here is in the specification of the indigenous language to be chosen as the official language of state business. Many Ghanaians would prefer to rely on a common indigenous national language but differ as to which it should be. Each would rather prevail, but many would rather coordinate on choosing an indigenous language than separate themselves from their cultural roots by relying forever on English as Ghana's official language.

EMPIRICAL EVIDENCE

The intuition behind this theoretical analysis is that there may be a Pareto efficient language policy in Ghana that has not yet been properly identified. But how are we to know whether an alternative dominates the status quo without knowing the subjective preferences of Ghanaians themselves? How are we to be sure that basic assumptions about language choice, embedded in the models, reflect social reality?

With these questions in mind, Edward Mensah and I drafted a survey and collected 39 responses from Ghanaians living in the Chicago area. The results were encouraging for the assumptions behind our models, and we reported them (Laitin and Mensah 1991). We redesigned the survey instrument and pretested it in Ghana in October 1990 while training graduate students to administer it. In December 1990, a revised interview schedule was administered to 300 respondents in six distinct regions of the country, so that we have interviews with 50 respondents each from the Dagbani, Ewe, Fanti, Ga, Kasem, and Twi language zones.

Interviewers (1) were indigenous to the area in which the interviewing was done, (2) introduced themselves alternately in the local language and in English and then asked the respondent which language he or she would prefer for the administration of the interview, (3) were given targets for people in certain social roles but without other provisions for representativeness, and (4) were encouraged—indeed, rewarded—for writing down in a separate book any side comments and criticisms that respondents offered during the interview.

The survey results give us a handle on some important issues in the political resolution of language coordination problems. I shall examine the assumption that the expected value of 100%-indigenous is higher than 100%-English and address the issue of side payments, a mechanism crucial for coordination games in general but apparently out of place in the realm of language choice.

Constitution of the Tipping Game

Establishing an individual payoff at 100%-indigenous that is higher than at 100%-English raises the thorny issue of calculating costs and benefits not only for learning a language, but in the sense of resentment that a majority of Ghanaians whose language is not chosen—and pride that the minority whose language is chosen—will feel if any indigenous language is chosen as Ghana's official language. The sense of pride in having an indigenous language as Ghana's official language must be weighed against the costs of developing sufficient teaching materials in a language that remains marginalized from international science and technology. And then there is the problem of administrative cost. The acceptance of English did not involve up-front costs for newly independent states. Meanwhile, the creation of mechanisms for reallocating costs in the imposition of an indigenous language is itself costly and should work to reduce the overall value of 100%-indigenous. For purposes of this preliminary discussion, the payoffs for a language choice are based only on the subjective assessment of the individual (or family) economic returns for having developed facility in a language and of psychological returns for having an official language that enhances social (or national) solidarity.

Respondents were invited to make these subjective calculations. Indirectly, a case can be made that ceteris paribus, the respondents would prefer to live in a Ghana under conditions of 100%-indigenous. However, there is some direct evidence against my assumption: on the simple question whether respondents preferred English to become the dominant language for official communication in Ghana or a single Ghanaian language to replace English, 60% chose English, and 40% chose the Ghanaian alternative.

The reasons for the preference for English need, however, to be analyzed. Some respondents who chose English pointed to the situation for small and relatively poor states in the international political economy. Citizens of those states must compete, respondents argued, for international jobs, and a solid education in English is an essential skill for successful penetration in the job market. As one respondent put it: "It would not have been any problem if Ghana were to have a vibrant economy that can take care of our needs. . . . The Japanese or Germans for instance don't need to learn someone else's language because you can grow and work in the economy of your country without having to travel elsewhere. What English does for instance is to offer us the opportunity to seek greener pastures elsewhere if it becomes necessary" (K16).

Other respondents who chose English articulated a belief that indigenous languages are best used for home life, to create solidarity, and to express emotions that simply cannot be done in a language that you cannot consider your own. For their sense of rootedness, they said, there need be only a few domains in which one uses one's indigenous language. A considerable number of respondents who chose to be interviewed in the language of home use found themselves unable to carry forward with the interview in that language and began using English. The interviewer of the Ga respondents noted that one person who chose to be interviewed in Ga "slipped" into English, and then "the answers started coming out" (G4). One Kasem respondent switched to English when he found the interview process to be "book long" and therefore more appropriately done in English (K32). Yet despite this loss of control over one's mother tongue for extensive conversation about national political issues, respondents would never consider giving it up for family functions.[12] The perspective from the international job market (favoring English) complemented by a view of the proper language for private domains (favoring one's mother tongue) would put a low value on the successful imposition of an indigenous language as the official language of Ghana.

But most of those who chose English as better than an indigenous language as the sole official language for Ghana did so because of practical problems in reaching a new equilibrium, not necessarily because the new equilibrium would be deficient. Many expressed the belief that no Ghanaian language is sufficiently "developed" in technical vocabulary or in scientific precision to play an important role in official communication. Others felt that although it would be good in the abstract, any language chosen would give its members too much "pride," which would be destructive to peaceful intergroup relations. The practical problems of implementation were so impressive as to color people's assessments of the payoffs should it be accomplished. As one respondent put it, "Even Kwame Nkrumah tried and it did not work so I do not think a single Ghanaian language can replace English as a dominant language for official communication" (G11).

Furthermore, the positive abstract support for a single indigenous language comes through clearly in the interviews. One respondent, before recounting the difficulties, said that "if there were a single Ghanaian as an official language its usage will have been ideal" (F23). Another pointed out: "One official language will identify us as a state. We have adopted English which has suppressed us" (G14).

Based on these data, I cannot provide an empirically justified expected value of 100%-indigenous and compare it with 100%-English, especially if values other than income are reckoned, such as pride and dignity and the chance to be citizens in a more powerful and coherent state. But I think it reasonable, given the reasons that respondents gave in their justifications for the choice of English as the sole official language of Ghana, to interpret these interviews as saying that transition costs aside, Ghanaians would prefer to live in a world of 100%-indigenous over 100%-English.

Will Side Payments Provide Incentives for Language Shift?

The usual way, theoretically, to induce movement from the status quo in an n-person tipping game is to provide selective incentives or side payments for particular individuals to change their language behavior (before the tipping point is reached) even though the average payoff for those who change will be lower than the average payoff for those who remain with the status quo.[13] Side payments involve compensating speakers of the nonchosen languages for their learning costs. Pool (1991) has demonstrated the theoretic possibility of accurately assessing costs (of learning and translation) so that the transition burdens can be made equitable. But he has not demonstrated that the costs of learning a new language and the costs of translation are the most salient costs in actual political cases. The psychological costs of personal complicity in the death of one's own language may be a principal concern of citizens.

The assessment of costs and benefits, our survey shows, takes place after the issue has been *framed.* Language shift can be framed as an efficiency measure or as a symptom of cultural death. Our data suggest that the big hurdle in implementing a policy through side payments has more to do with these "framing effects" than it does with getting the learning and translation costs right (Goffman 1974; Tversky and Kahneman 1986).[14]

In some frames, costs do not seem to matter. The costliest solution in Pool's analysis for "losers" is for them to learn the newly chosen language without compensation from either the winners or the state. Yet respondents had little trouble accepting as normal the costs when they were framed in a diffuse way. We asked: "If Ghana were to choose a single African language as the standard language for all official, educational matters and it was one from a region different from yours, would you (a) Seek to learn that language quickly to enable you to understand official speech? (b) Organize to get for your own language equal recognition? (c) Other (please specify)?" On this question, 226 out of 294 respondents (77%) said they would seek to 'learn' the language. Put this way, respondents make the arbitrary choice of an indigenous language as official look like a good (and cheap, for the state) idea.

Side payments suggest a different frame. When we proposed them, respondents were less positive about accepting the idea of the imposition of an indigenous language foreign to them as Ghana's official language than when we asked if they would learn or organize. We asked a series of questions as to whether they would be willing to accept a Ghanaian language other than their own as the medium of instruction in their children's school if the students were given job guarantees by the state for having made the language sacrifice. Only 26% of the respondents were willing to take any side payment offered; 31% were willing to take side payments only when considered useful; and 43% were adamantly unwilling.[15]

Many respondents could not take the job promises seriously. Either they had no trust that their government would fulfill such promises, or they calculated that the expected value of a government job (without knowing English well) was far less than the value of English literacy with the enhanced value of international migration. One respondent said, "If the child has no English he would be permanently handicapped in his search for a job particularly in the international labour market" (K21). A woman who spoke no English and was willing to accept tax transfers that virtually no other respondents could abide answered *no* to the job incentive: "To get a 'white man's' job to do, you must speak and write English and since that is what we all want our children to do, I will like him to study English well" (K43). Some respondents clearly saw that the international labor market offered better prospects than did the Ghanaian government, whose promises few respondents would take seriously. One interviewee responded to the job incentive by saying: "Your questions are funny. If he learns no Akan and the government does not give him a job, he will farm" (D2).

If job incentives had some takers, our proposal that tax transfers be used to compensate losers had virtually no takers at all. In these questions, we mentioned *specific* languages (Akan, Hausa, and Ewe) as chosen languages. Some respondents said *no* on expected value calculations. For example, a respondent from outside the Akan-speaking zone mentioned Akan earlier in the interview as the obvious choice for a national language. Yet he rejected the job offer for his children if they were to study in the Akan medium "because . . . the money involved in learning Akan will be greater than the tax payment" (K50). In a similar vein, another respondent claimed, "I will prefer paying the tax to learning any other language as it is more expensive to learn language" (G3).

Most respondents did not perceive this question in terms of utility but, rather, in terms of morality. They saw these questions in the frame of a nefarious bribe and refused even to calculate: "This is pure bribery, it can never work, not here alone, all over the country" (E25). Many respondents, after hearing this offer, excoriated the interviewer for even posing the question—or for working for a biased scientist or for supporting an underground project to impose a foreign culture on them: "Is the fellow who designed the questions an Akan? He is biased" (D12). Chauvinistic comments about the respondents' own languages were brought to the fore: "We can't allow our language to die; it will mean the death of our very essence" (K48); "To neglect my language for another person's language, over my dead body" (F8); "I don't want my language Fanti to die a natural death and lose my cultural identity" (F7). Complementing the chauvinism and showing a reversal of the easy toleration most respondents demonstrated when they agreed to learn the language of the chosen language on their own, some respondents showed a disdain for the mentioned languages:[16] "Why do you force Akan on us. This is annoying" (D9); "I know they want us to accept Akan. But let me tell you this is a dream. It is going to bring war if the government should force Akan on the people" (D14); "Even if they force Akan on us, we will not learn" (D28); "Akan is a thing I detest. I prefer English" (G40). When non-Ewes were questioned about Ewe, the reverse feelings were expressed. One Twi respondent argued that Ewe should not have been mentioned at all: "No Akans feel good about it" (T20). A Kasem respondent rejected the tax proposal for learning Ewe: "Ewe is not known here; what is more, the Ewe people are not liked by most tribes in Ghana" (K13). In sum, the frame that the tax question put around the language picture is graphically exemplified by a Ga respondent who pleaded, "I cannot sell my birthright" and then criticized the interviewer: "You just can't kill one language and use it as a manure for another" (G14).

Much more work in analyzing our present data and collecting new data will be necessary to understand how language incentives can be successfully framed. Our interview schedule did not allow us, for example, to distinguish whether it was the idea of a tax or the mention of a specific language that evoked strong emotional outbursts. None the less, the interviews demonstrate that people indeed make expected value assessments in regard to language but that framing effects are powerful. We have seen that people are more amenable to diffuse than to specific commitments in regard to language investments. Further, we have seen that people have clear notions about inappropriate incentives for changes in their language repertoires that are not guided by issues of price. Incentive schemes for language change that do not factor in these framing effects are likely to trigger popular responses that will appear irrational to policymakers.

CAN THE 100%-ENGLISH EQUILIBRIUM BE UNDERMINED?

The Market Place of Languages Without Direct Political Interference

The present equilibrium based on the Schelling diagram approaches 100%-English, where all the official government documents, schools, and big businesses operate in English. This, of course, is a rather stylized view of the language repertoires that individual Ghanaians must master if they are to negotiate their ways through job, family, and the marketplace. As we saw, vernaculars and code-switching speech forms make for a complex language scene. And despite the equilibrium at 100%-English, virtually no Ghanaians living in Ghana have brought up children who are ignorant of at least one Ghanaian vernacular. The costs of maintaining the vernaculars (at least as languages for family and personal matters), are low; and the risks of abandoning them, given the possibility that order could break down in the cities and the people go back into ancestral villages for protection, fairly high.

From the point of view of transaction cost analysis, this solution to the Tower of Babel curse does not look too bad. Ghanaians must know two languages (English and their own vernaculars) in order to communicate with most local residents and all formally educated citizens in the country. But English also gives them access to virtually all important international communication networks, so English helps kill two birds with one stone.

From the point of view of the creation of a national culture, however, this solution has a serious deficiency. Since language is often considered an essential element of a national culture, reliance on English for intracountry communication gives Ghanaians the sense that while other countries (Sweden, Netherlands, Spain, etc.) have national cultures, they lack one. An internal (to the state) solution to the inefficiencies of a multilingual society would, in the eyes of many articulate nationalists, be preferable to the status quo.

Work on the optimum size of firms suggests there may be an economic as well as a cultural logic supporting the investment in indigenous national languages (Williamson 1986). First, organizational decentralization permits greater levels of experimentation and competition. And a world of language-specific countries allows for a greater degree of decentralized decision making than would be the case if international transaction costs were equal to intranational transactions costs. Second, for a country making an investment in national education, language-specific curricula lower the probability of "brain drain," as it encourages highly trained personnel to stay in their country to work, so that their children can get a firm grounding in the parents' language. Third, a reputation for having a legitimate national culture with a loyal workforce is also useful in attracting international investment.[17] To be sure, there are other ways than language specialization to establish a loyal "brand-name" national culture (Switzerland and the United States have succeeded here), but language specialization is an extremely powerful method to differentiate a national population from foreigners.

The Lottery: A Standard Microeconomic Solution to Coordination Dilemmas

However rational for the country, coordinating on an indigenous official language is irrational for the individual. First, as the tipping model suggests, it is irrational for any Ghanaian parent to seek education for a child in an indigenous language unless a great majority of other parents already had done so. A second reason is that even if the government did choose an indigenous official language, speakers of the nonchosen languages would have a dominant strategy to subvert the policy, and speakers of the chosen language, knowing this fact, would follow suit. Table 1 illustrates why this is the case.

TABLE 1

The Akan–Dagbani Coordination Game with Akan as the Chosen National Language

DAGBANI PARENT'S CHOICE AS MEDIUM OF INSTRUCTION FOR CHILD	AKAN PARENT'S CHOICE AS MEDIUM OF INSTRUCTION FOR CHILD	
	ENGLISH	AKAN
English	5, 5	4, 2
Akan	1, 4	3, 8

Table 1 (simplifying the situation by assuming only two competing indigenous languages) portrays the strategic situation for an Akan and a Dagbani parent in the face of an announcement of a standardized Akan as Ghana's official language and the medium of instruction in all public schools. This program presents a dilemma for Ghanaians. Should they enroll their children in government schools or pay a higher tuition and enroll their children in one of the private, English-medium schools that begin to appear in every town? How should they calculate their payoffs for each choice? If all send their children to the government schools, the route toward cultural independence will have been paved. But if all send their children to English-medium private schools, the government program (however popular in the abstract) would be subverted. But if any parent sends children to the government school while most others send their children to private school, that parent will have lost the game of coordination.

For Table 1, let us assume a utility of 5 if both send their children to a private, English-medium school. If they do not coordinate, the one who learns English gets a 4, while the one who gets educated in one's own indigenous language gets a 2, and the one who learns in a foreign indigenous language gets a 1. With Akan being the chosen language, we give an 8 for the Akan in the "Akan–Akan" cell. Let us say that for the Dagbani parent, education in Akan, under conditions of coordination, would yield 3, which is higher than noncoordination but lower than coordination in English. Under these circumstances the Dagbani player would have a dominant strategy to choose English, and the Akan player would rationally follow and choose English. The unique equilibrium is therefore "English × English." Given this situation, it would be impossible for a government to get noncoerced compliance for the promotion of any indigenous official language.

For coordination problems of this sort, game theorists have devised lotteries. For example, in the battle of the sexes, each player would prefer a coordinated coin flip to avoid the possibility of an uncoordinated outcome. Because game theorists rely on lotteries to solve coordination dilemmas, it is useful to think how such a device would work to solve the Tower of Babel

TABLE 2

The Akan–Dagbani Coordination Game under Conditions of a Lottery for National Language Choice

DAGBANI PARENT'S RESPONSE TO LANGUAGE LOTTERY	AKAN PARENT'S RESPONSE TO LANGUAGE LOTTERY: REJECT	AKAN PARENT'S RESPONSE TO LANGUAGE LOTTERY: ACCEPT
Reject	5, 5	5, 5
Accept	5, 5	$(1 - p)y + p3$, $px + (1 - p)3$

Note: p = probability of Akan's winning lottery; $1 - p$ = probability of Dagbani's winning the lottery; x = utility to Akan speakers if Akan won the lottery; y = ultility to Dagbani speakers if Dagbani won the lottery.

curse. Consider Table 2, where the Dagbani and the Akan parent are each asked to enter a language lottery in which a single Ghanaian language would be chosen as official. (In this case, the government would put chits into an urn, each labeled with a language, in proportion to the percentage of the population, and then choose a chit blindfolded.) Players can either accept or reject the lottery. If either rejects, the status quo of all-English is retained, in which case each player receives a score of 5.

The probability (p) that Akan will be chosen, given the higher percentage of Akan speakers, is greater than the probability ($1 - p$) that Dagbani will be chosen. However, the utility if Dagbani is chosen for a Dagbani (y), given the low percentage of Dagbani native speakers and the large employment opportunities available for those who have facility in the official language of state, is greater than the utility for an Akan (x) if Akan is the lottery winner. Assume a score of 3 for the lottery loser, which is lower than coordination in English, but higher than a failure at coordination. The expected utility for an Akan for playing the lottery is $px + (1 - p)3$. The expected utility for a Dagbani for playing the lottery is $(1 - p)y + p3$. The lottery would be a rational outcome if both expressions yielded payoffs greater than 5. (If $p = .7$, $x = 6$, and $y = 10$, for example, players should rationally enter the lottery.)[18]

The problem with this lottery solution is that it assumes lottery players will respect its results. But how can either player credibly commit to accepting the lottery outcome? Why should any player believe that the loser would not subvert the lottery outcome after its announcement? Even if parents committed their children to matriculation in a government school as a precondition for playing the lottery, these parents could send future children to English language schools and subvert the lottery outcome in the long run. While the lottery principle is useful to unhook us from seeing the status quo as inevitable, it does not provide a compelling answer to the problem of coordination on an indigenous language in multilingual countries.

The 3 ± 1 Language Repertoire as a Politically Induced Equilibrium

Perhaps the dream of indigenous language promotion is quixotic? The logic of the tipping game, the probable backfire of economic incentives aimed to induce language shift, and the difficulty of assuring commitments in a language lottery all suggest the power of the status quo. Ghanaians, it seems, will continue to rely upon English for administrative, technical, and international communication, while using their vernacular languages for communication in informal domains with other Ghanaians who share that language.

Yet the models so far presented all assumed that there was a binary choice between English and some indigenous language. None of the models examined the strategic possibilities of multilingualism. For most of Africa, unlike America, multilingualism is the norm (Kashoki 1977). The de facto solution to the Tower of Babel curse has been the investment in multilingual repertoires. The final question that I shall address is whether a Ghanaian government could, in the context of a multilingual policy, promote a single indigenous language as official.[19]

Indeed, respondents favored a multilingual context and were receptive to policies that promoted different languages in different domains of use. To be sure, they did not want a form of multilingualism where there would not be a common means of communication. When asked if they preferred for each group in Ghana to promote its own native language or whether Ghanaians should agree on a single Ghanaian language as the major language of official business, a majority (59% to 39%) chose the latter. But vernacular development, along with English as a lingua franca, was highly favored. When asked whether they preferred for English to become the dominant language in all walks of life or for English and the indigenous languages to share importance, a significant majority (89% to 9%) chose the latter. These respondents had a clear notion of how language domains should be ordered. For district council political administration, 72% favored the vernaculars as the official medium, where only 28% chose English. For medium of instruction in primary school, however, 87% favored English, while 10% favored the vernacular. While the political yearning for a unique national language could not be realized in the framework of a one-language state, perhaps it could be realized within a multilingual framework?

In light of these preferences, a plausible multilingual model for Ghana is that of India (Laitin 1989). Throughout postcolonial India, English remains the principal language of education, business, and political administration. However, the all-union (i.e., federal) government promotes Hindi, and this language is important for interstate official communications and popular culture (films, music, television). The state governments each promote state languages, and these languages are media of instruction in primary education and are used extensively in state-level administration. Citizens need to know English, their state language, and Hindi to move in a variety of social, political, cultural, and economic circles. Those whose states rely on Hindi need to learn only two languages; and those who speak protected minority languages within states that have a different language as official need to learn four. This outcome can thus be called a *3 ± 1* formula.

Ghana, as we have seen, does not have a language such as Hindi to serve as a credible national language. Countries such as Indonesia (with Bahasa) or Kenya (with Swahili) are more obviously able to follow the Indian model. Pidgin might serve that role for a number of West African states but not (due to its limited use and extremely low prestige) in Ghana. Ghanaian, however, if given recognition by linguists, authors, and national politicians, might play such a role.

If Ghanaian is not seen as threatening to any group—with district leaders getting rents from the promotion of the regional vernaculars (by appointing officials literate in the vernacular giving vernacular speakers chances to provide translation services, and by advertising tenders in that language) and parents getting education in English for their children at government schools—the seeds might get planted for an indigenous official language. If Ghanaian were recognized, in a context in which district languages had important roles and where English remained crucial in technical and administrative domains, the macro outcome would be called a *3 + 1* formula (not 3 ± 1, since no district language would be Ghanaian). To be sure, there is now no literature, no texts, and no name for this language; Ghanaian could not be accepted as a substitute for English in a binary choice atmosphere. It would have to emerge out of a long succession of micro choices.

Ghanaian would displace neither English nor the district languages in the short term but would be an outlet for artists, novelists, television scriptwriters, and translators, all of whom could be subsidized for their literary production. If Ghanaian is really the language of the growing urban popular classes, its chances of evolving into a national symbol are greater. It would need its Dante, or Webster, or perhaps Shakespeare for ultimate legitimation; but the government could set the stage for the emergence of such a figure.[20]

This 3 + 1 equilibrium would give all Ghanaians the ability to communicate efficiently (without translators) with each other, and Ghanaians would study English sufficiently in school so that they could communicate internationally as well. The education costs for this equilibrium would be higher than the equilibrium at 100%-English, but there would be gains in human dignity (people using their own language in high-status domains) and in the creation of a national culture, which Ghanaians say they want, even though coordination dilemmas have up until now forestalled any effort to envisage how that culture could be created.

The Electoral Basis for a 3 + 1 Outcome

The 3 + 1 outcome could not result from a free market of language investments, without government interference. Even if it could be demonstrated that it was a Pareto superior outcome to the status quo (something that has not been demonstrated here), there would still need to be a government with the will and motivation to act. What evidence is there that such political engineering is likely, given the high incentive for any government to avoid the risk of intergroup antagonism by keeping the language issue off the political agenda?

In fact, the data in our survey point to the risks of such a program rather than its likelihood. Even those respondents whose children were disadvantaged by the all-English curriculum (in that children whose parents already spoke English had an easier time learning in school) wanted English as the medium of instruction in their children's school. They were willing to allow their vernaculars to be used in private domains, so that the strong respondent support of English along with vernaculars over the sole use of English reflects a desire not for equality between languages but rather for the symbolic recognition of the vernaculars. And most people fear the consequences of the arbitrary choice of one vernacular as the sole official language of state. English plays a role in Ghana similar to that of foreign kings invited to rule over small European states in the nineteenth century: it assures neutrality.

Nonetheless, support for an indigenous national language is a natural element in a populist opposition program. Populism can exploit a general feeling throughout a population that the only people who are profiting from the government are the bureaucrats and executives in multinational corporations and international agencies, all of them distinguished by their excellent command of English. Under these conditions, a populist contender might find that a program of cultural nationalism can capture the imagination of disgruntled voters. The dominant message would be a return to cultural and linguistic roots of Ghana. To be sure, populism in Africa for the most part has been associated with the political movements that celebrate the separate identity of particular language groups. Would the politicization of language issues in Africa promote just this sort of populism where first provinces and then districts would seek national sovereignty, leading in the limit to the absurdity of each idiolect's having its own country? Maybe so. However, there may exist populist strategies supporting the promotion of indigenous languages that are not divisive.

Suppose a populist challenger wins a national election in part on a platform of cultural independence and the need for a Ghanaian national language. Ghanaians might well vote for such a program (just as many Americans supported prohibition) with the full intention of subverting it privately. Such subversion would assure an English-medium education for their own children while all other children are receiving a curriculum with an indigenous language playing a central role. Their own children would then be well equipped to land the best white-collar jobs in Ghana. The cumulative effect of such reasoning—which might be called the private subversion of a public good—would be the failure of the national program.

To implement such a program without mass subversion, the populist leader would need to assure district leaders that their languages would not be undermined by the promotion of a single indigenous language and to assure parents that English would remain central to the curriculum. One possible plan would be for the new leader initially to introduce a curriculum in the state schools that included English and district vernaculars as dual media of instruction. Those parents (constituting 89% of our respondents) who preferred English along with the vernacular over English alone would not likely withdraw their children from state schools as long as English remained central to the curriculum. Furthermore, district leaders would be pleased to implement the program if only because the promotion of district vernaculars would give local leaders patronage power (e.g., in purchasing text books) and the ability to keep local administration in the hands of the people who speak the district vernacular.

Meanwhile, the national leader could begin supporting the development of pedagogical materials in a national language, let us say Ghanaian. (This includes more than textbook production. National programs to promote a literature in Ghanaian would need to be implemented, say, by subsidizing books and newspapers that use it.) After a period in which the vernaculars had been institutionalized in district schools and materials developed in Ghanaian, the leader would be in a position to introduce the proposed national language into the curriculum. District leaders, who may have fought such an introduction in schools that relied only on English and fought the simultaneous introduction of a national language and vernaculars in fear that the latter would dominate the former, would be less inclined to fight against the introduction of Ghanaian when the schools were already promoting district languages. Parents who feared the development of district languages without a national language (59% of our respondents) would find this development a healthy one, as long as English remained dominant as a medium of instruction. And since Ghanaian represents no particular subgroup in Ghana, the communal animosities that appeared when we asked respondents about Akan or Ewe as a national language would be side-stepped. Few parents would have an incentive to take their children out of the state school system.

The long-term result of this strategic introduction of languages into the national educational curriculum could be a universally understood indigenous Ghanaian national language, one that was introduced without coercion, without intercommunal conflict, and without mass subversion of public institutions.

Much of my thrust here has been to demonstrate the difficulty of attaining what may be a Pareto superior outcome for countries such as Ghana, where there is a strong popular desire for the implementation of a national language but an inability to do so, in large part due to the difficulty of reaching a consensus as to which of many vernaculars could serve as a national language. Even if the national language does not (at first) have high status, part of the solution must be in the choice of a neutral national language, largely because the use of a lottery to choose an already-established vernacular as the national language will break down. As a populist leader develops a neutral language as the national language, that leader can strategically work for its acceptance. The political program I have illustrated shows that the inchoate desires of many Ghanaians for an indigenous national language are not hopeless.

CONCLUSION

The curse of the Tower of Babel afflicts African states in general, and Ghana in particular, with great force. I have sought to think systematically about a solution that improves upon the market-induced outcome, which, from the point of view of many Ghanaians, is inefficient. I have assumed (reasonably, as the surveys show) that Ghanaians themselves would prefer coordination on a language from within the country over one of a previous colonial regime. I have shown the underlying reasons why coordination is such a problem. By doing so, I was able to suggest alternative strategies for reaching a coordination point preferable to the one that the status quo now offers. This is the 3 + 1 language outcome, in which an indigenous national language gets promoted and institutionalized, along with district vernaculars—all in the context of continued reliance in many domains on the language of former colonial rule. This outcome would be favored over the status quo by many people in countries like Ghana, where there is societal multilingualism and a coordination problem for choosing a national language. I have provided a strategic analysis of how coordination problems can be overcome to attain a 3 + 1 outcome. However, I have not here offered a definitive solution to the curse of Babel but rather provide an analytical representation of the problem that makes it more susceptible to solution.

Notes

This paper was originally prepared for the annual meeting of the American Political Science Association, Washington, 1991. The Spencer Foundation financially supported the fieldwork for the empirical research reported herein. Edward Mensah is my collaborator in the larger project concerning language in Ghana, of which this paper is a part. The survey project in Ghana was managed by A. Afrifa and Pwasanga Ataogye, and their expertise in psychology added depth to our interviews. James Fearon provided invaluable technical and theoretical support. Ronald Rogowski and Duncan Snidal

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1. It is unfortunate for me that Brams did not consider the Tower of Babel in his stimulating *Biblical Games* (1980). On issues of coordinating on an official language, Pool's models (1991) have been pioneering.

2. A lingua franca is a language of a certain group appropriated for intergroup communication. A pidgin is a mixed language that is not native to any group and is not passed on intergenerationally as a first language.

3. Transaction cost concerns not only help us interpret the behavior of state rulers but also help explain language investments of individuals. In situations where there are pockets of monolingualism in a language different from the language of rule, certain individuals, by investing in an expanded language repertoire, have reduced transaction costs for their own affairs and have taken advantage of transactions barriers that others face by providing useful translation services (De Swaan 1988).

4. For the reasons why twentieth-century state building provides greater difficulties for rationalization, see Laitin 1992, 38–45. For the set of multilingual African countries without a common indigenous lingua franca, see Laitin 1992, 121–32.

5. Interview conducted by David Laitin with Kwamena Ahwoi, Provisional National Defense Council secretary for local government, Accra, 29 October 1990.

6. For purposes of this study, I commissioned K. K. A. Anti to undertake a survey of 22 schools at all levels in Cape Coast, a city that often prides itself on having the most modern educational system in Ghana and one that they feel should serve as a model for all of Ghana. The wide variety of elite Ghanaians who send their children to boarding school there attests to the national acceptance of their claims.

7. Pidgin in West Africa is heavily influenced by English, but relies on indigenous language structures and vocabulary as well. It is socially perceived as an indigenous speech form.

8. Respondent numbers are indicated in parenthesis. The letter refers to the initial of the sample group's vernacular, to be identified later on in the text.

9. Code switching, according to Haugen is "the alternate use of two languages, including everything from the introduction of a single, unassimilated word up to a complete sentence or more into the context of another language" (1978, 21).

10. Schelling's style is to present interesting-looking payoff curves and then match the pictures with actual situations, even if "there is no guarantee that a pair of real choices exists somewhere that corresponds to some pair of curves that we might adopt on heuristic or architectural grounds" (1978, 219, 234). My approach is to draw the payoff curves consistent with my sense of the subjective utility of actors in a specific situation, even if the curves lack Schelling's cleanness. Two aspects of the curves in Figure 1 have interesting features: (1) the tipping point k is closer to 100%I than 100%E; and (2) in the movement toward the 100%I equilibrium, the declining marginal return for English bottoms out, and the value of E begins to rise as the society moves toward 100%I. Neither directly affects my argument here; I include these features in the figure as a reminder that the game is more complex than what can be portrayed here.

11. That is, the expected utility for speaker i is the sum for all members of k of the utility of each language if it were chosen as official multiplied by the probability of its being chosen. For Figure 1 to be empirically based, we would need data on expected utilities for all individuals at each point on the horizontal axis.

12. Dorian (1981) has developed a "tipping model" showing how as the number of domains (e.g., work, family, recreational reading, poetry) in which a language is used decreases, a dynamic leading to "language death" will likely be set in motion. Among the Ghanaian respondents to the survey recounted here, the tenacity concerning one's indigenous language (especially in the domains of family and community life) makes the "tip" toward death highly unlikely. More than feelings of local pride are at stake. Spouse selection, ability to cement support networks in cosmopolitan centers, and the opportunity to get a chief's title in one's hometown are all in part contingent on maintaining facility in one's home language. The observation that local languages and dialects can serve well-demarcated needs over generations, without facing death, has been made by Fernandez (1986).

13. Another theoretical tack is to introduce sequencing, building on the assumption that certain individuals have higher-than-average payoff for I or lower-than-average payoff for E at some point to the left of k. Any individual who, by virtue of "switching," pushes the game closer to k could "trigger" the move of a second individual, whose payoff for I now is higher than that for E. A version of a sequencing model will be presented in the final section.

14. I use the word *framing* somewhat more broadly than do Tversky and Kahneman: They require that the alternatives have the same expected utility. Some of the respondents in the survey reconfigured the utility for certain options based on factors external to the situation presented to them. For this reason, my use of the frame concept is closer to Goffman's than to Tversky and Kahneman's.

15. I do not here address the issue that while side payments may equalize costs, the provision of government sinecures as a side payment may have the effect of reducing significantly the gross national product and thereby reducing the average payoff for 100%I. Nor do I address the issue of the administration of side payments that may take on characteristics of welfare bureaucracies and thereby enrage the population. The specter of bureaucratic–linguistic tyranny could raise the subjective utility of the status quo at 100%E.

16. Rarely did these emotions surface until interviewers got to the question, at the end of the interview, about the tax proposal. But one exception was a Dagbani respondent who, in response to the question asking for a suggestion as to which language might serve as an indigenous national language, responded in anger: "For a long time now, the Akans think that we northerners are less human. If we adopt their language, our position is going to worsen" (D30).

17. This notion is by analogy from David Kreps's (1990) arguments providing a rationale for the investment in distinct corporate cultures.

18. If utility is modeled as a linear function of group size, it would be possible to predict whether any group would rationally enter the lottery. The slope of the function would determine whether the risk were superior to the status quo for each group. Some functions would favor groups on the extremes (very large and small populations); other would favor groups that had population percentages closer to the median group.

19. In Laitin 1992, the model that follows is formalized and applied to a large set of postcolonial states.

20. Weinstein (1979) refers to such types as "language strategists."

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CIVIC CULTURE AND DEMOCRACY: THE QUESTION OF CAUSAL RELATIONSHIPS

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A causal model of relationships between structural properties of states, civic culture attitudes of the general public, and change in level of democracy is tested with cross-national data. The model permits inferences about the possibility of unidirectional or reciprocal causation between civic culture attitudes and democracy, controlling for macrosocietal variables such as economic development, income inequality, and subcultural pluralism. Most civic culture attitudes do not have any significant impact on change in democracy. One of them, interpersonal trust, appears clearly to be an effect rather than a cause of democracy. The exception is the percentage of the general public that prefers gradual reform of society instead of revolutionary change or intransigent defense of the status quo. Support for gradual reform has a positive impact on change in democracy, and it is unrelated to a country's years of continuous democracy—findings that support the hypothesis of a unidirectional civic culture effect on democracy.

An important unresolved question in the study of democratization is whether attitudes of the general public have a major causal effect on the establishment and stability of democratic regimes. The theory of civic culture proposed by Almond and Verba (1963) and further elaborated by Inglehart (1988, 1990) postulates that the viability of democratic institutions is affected powerfully by attitudes such as belief in one's ability to influence political decisions, feelings of positive affect for the political system, and the belief that other citizens are basically trustworthy. Countries with high levels of these civic culture attitudes are expected to be more likely to adopt and sustain democracy over time than countries with low levels, regardless of socioeconomic factors such as level of economic development. An alternative possibility is that civic culture attitudes are an *effect* rather than a cause of democracy. According to this line of argument, democracy typically is established for reasons other than civic culture attitudes of the general public, and the successful persistence of democracy over time is likely to cause increases in levels of civic culture attitudes because high levels of subjective political competence, pride in the political system, and interpersonal trust are a rational, learned response to the experience of living in a country that has a stable democratic regime (e.g., see Barry 1978, 50–52; Schmitter and Karl 1991, 82–83).

The hypothesis that democracy causes civic culture would seem a priori to be as plausible as the hypothesis that civic culture causes democracy. If so, causal models of the relationship between civic culture attitudes and democracy should be formulated to take into account the possibility of reciprocal causation between these variables.[1] Until recently, it has been difficult to assess the relevance of attitudes of the general public for the establishment of stable democratic regimes because of lack of sufficient cross-national data on attitudes. The pioneering study in this regard was conducted by Inglehart (1988, 1990), who was the first to compile data on attitudes of the general public for a sample of countries that was large enough to permit multivariate statistical analysis of the relative influence on democratization of micro political culture attitudes as compared with macro socioeconomic factors. Inglehart's causal model is grounded, however, in the assumption of unidirectional causation—that civic culture has an effect on democracy and that democracy does not have an effect on civic culture.

Inglehart's model consists of four variables: (1) a country's level of economic development in 1950, as measured by its gross national product (GNP) per capita; (2) the percentage of the labor force employed in the tertiary sector (no date given), which is interpreted as an indicator of the size of the middle class; (3) a composite measure of civic culture over 1981–86 that reflects an average for the general public of its level of life satisfaction, interpersonal trust, and lack of support for revolutionary change; and (4) a country's years of continuous democracy from 1900 to 1986. The model stipulates that economic development is a predetermined variable (which may have causal effects on the other three variables) and that the percentage of the labor force in service occupations (the tertiary sector) and the average level of civic culture are intervening or mediating variables, caused by variation in level of economic development, which, in turn, are causes of variation in a country's years of democracy. Estimates of the effects of the predetermined and intervening variables on democracy showed that economic development had no significant direct effect, that the labor force in services variable had a direct effect of +.36, and that the civic culture variable had a direct effect of +.74 (Inglehart 1990, 44). These results led Inglehart to infer "that over half of the variance in the persistence of democratic institutions can be attributed to the effects of political culture alone" (p. 46). And he concluded more generally that this evidence "tends to confirm the basic thesis of *The Civic Culture*" (p. 48).

If Inglehart's causal inferences are valid, explanations of democratization that emphasize political culture attitudes must be given primacy over explanations that emphasize the importance of macro socioeconomic conditions. The problem is that the possibility of an effect of years of continuous democracy on civic culture attitudes is ignored. A proponent of the alternative hypothesis that democracy causes civic culture attitudes could reasonably argue that the supposed "effect" of civic culture on democracy is really an effect of democracy on civic culture. Indeed, because of the fact that a country's years of continuous democracy are calculated from 1900 up to the time of the measurement of the civic culture attitudes during 1981–86, this means that the temporal status of the *years of continuous democracy* variable is *prior* to the civic culture measurements. It would thus be more straightforward and plausible to interpret a strong association between civic culture 1981–86 and years of democracy 1900–1986 as reflecting the possibility of a strong causal effect of the persistence of democratic institutions on a country's level of civic culture.

A second problem with Inglehart's analysis involves the conceptualization of civic culture, defined operationally by a composite index of life satisfaction, interpersonal trust, and lack of support for revolutionary change. Since life satisfaction is not a component of the civic culture concept originally proposed by Almond and Verba and since Inglehart does not provide a theoretical justification for why life satisfaction should be assumed to be part of a unidimensional syndrome of civic culture attitudes, one could argue that life satisfaction should be analyzed separately from interpersonal trust and lack of support for revolutionary change. Moreover, since these latter variables also are conceptually distinct and cannot be assumed on definitional or empirical grounds to be unidimensional, it would be useful to analyze them separately, too. Therefore, the more appropriate measurement strategy would be to "unpack" the composite index of civic culture and test hypotheses using each of the attitudinal variables separately.

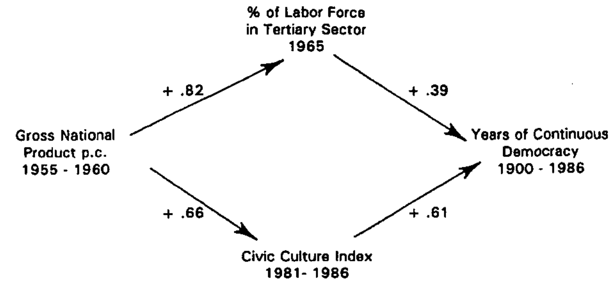
A potential third problem is that Inglehart's model may be subject to specification error. Inglehart assumes that the percentage of the labor force in service occupations is the only relevant macrosocietal determinant of democracy apart from level of economic development. Given the fact that the *labor force in services* variable has never been included previously in cross-national macro research on determinants of democracy, this strong assumption seems dubious. Omitted variables for which a compelling case can be made theoretically, following the analysis of Dahl (1971), include income inequality and subcultural pluralism. Empirically, research focusing specifically on the stability of democracy over time has found that the extent to which income is distributed unequally is an extremely powerful negative influence that mediates the positive effect of economic development (Muller 1988). And research on determinants of cross-national variation in level of democracy has found that subcultural pluralism is one of the relevant noneconomic influences (Bollen and Jackman 1985). Income inequality and subcultural pluralism may thus be relevant omitted causes that could affect the estimates of causal influence in Inglehart's model.

Although we are critical of Inglehart's model specification and measurement procedures, we share his concern with assessing the relevance of attitudes of the general public for the establishment of stable democratic regimes. The step that needs to be taken now is to formulate a research design that enables one to address more effectively the question whether unidirectional or reciprocal causal relationships may obtain cross-nationally between civic culture attitudes and democracy, controlling for the influence of macrosocietal determinants. We shall test an alternative causal model in which the dependent variable is change in a country's average level of democracy from the 1972–80 period to the 1981–90 period. If civic culture attitudes have significant effects on change in democracy, such that high levels of civic culture attitudes are associated with increases in democracy and low levels are associated with decreases in democracy, then at least the possibility of civic culture causing democracy will be supported. By contrast, if civic culture attitudes do not have significant effects on change in democracy, then one can be skeptical about claims that civic culture attitudes are an important cause either of transitions from authoritarian rule to democracy or of the persistence of democracy over time. The possibility of a causal effect of democracy on civic culture attitudes will be taken into account by including a country's long-term experience of democracy in the model as a predetermined variable that could have an influence on civic culture attitudes independent of other macro socioeconomic conditions.

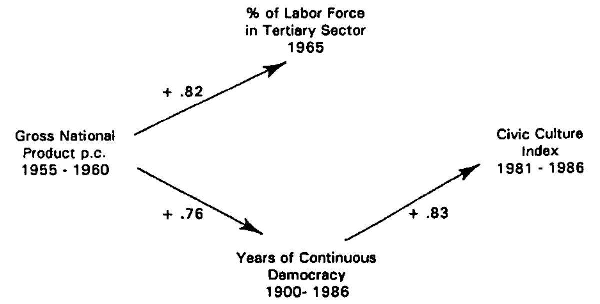
We begin our analysis by testing for causal relationships between civic culture attitudes and democracy using Inglehart's sample of primarily European (or European-heritage) countries. Then we test an alternative causal model with an expansion of Inglehart's data set. We have collected measurements of system affect (as inferred from attitudes about societal change) and interpersonal trust (elements of civic culture common to the conceptualizations of both Almond and Verba and Inglehart) from representative surveys conducted during 1990–91 of urban populations in the six Central American states: Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama (see Appendix B). These data are added to the measurements used by Inglehart from surveys conducted during 1981–86 (see Appendix A).

FIGURE 1

Path Coefficients for Two Specifications of Inglehart's Model of Economic and Cultural Determinants of Democracy (N = 20)


% of Labor Force in Tertiary Sector 1965
+ .82
+ .39
Gross National Product p.c. 1955 - 1960
Years of Continuous Democracy 1900 - 1986
+ .66
+ .61
Civic Culture Index 1981- 1986

A. Inglehart's Specification Assuming Civic Culture Attitudes Are Causally Prior to Years of Continuous Democracy


% of Labor Force in Tertiary Sector 1965
+ .82
Gross National Product p.c. 1955 - 1960
Civic Culture Index 1981 - 1986
+ .76
+ .83
Years of Continuous Democracy 1900- 1986

B. Alternative Specification Assuming Years of Continuous Democracy Are Causally Prior to Civic Culture Attitudes

CAUSAL MODELS OF ECONOMIC AND CULTURAL DETERMINANTS OF DEMOCRATIZATION

Estimates of causal path coefficients for versions of Inglehart's model of economic and cultural determinants of democracy are shown in Figure 1. The measure of civic culture is a composite index of life satisfaction, interpersonal trust, and opposition to revolutionary change constructed for 20 countries with information on all three variables.[2] The results obtained for model A closely reproduce those reported by Inglehart (1990, fig. 1–6).[3] In this model, the index of civic culture measured with data from 1981 to 1986 is assumed to have a unidirectional effect on a country's years of continuous democracy since 1900. Given that assumption, the path coefficients indicate that civic culture attitudes are by far the most important determinant of years of continuous democracy, that the percentage of the labor force in service occupations (the tertiary sector) has a much smaller direct effect on years of continuous democracy, and that the influence of level of economic development is entirely mediated by civic culture and the percentage of the labor force in services.

Model B shows the results when the temporal ordering of civic culture and years of democracy is corrected so that years of continuous democracy from 1900 to 1986 precedes civic culture attitudes observed for 1981 to 1986. This model shows strong support for the alternative hypothesis that the experience of stable democracy produces high levels of civic culture. When the civic culture index is regressed on years of continuous democracy, percentage of the labor force in services, and economic development, only *years of continuous democracy* has a statistically significant effect.[4] Moreover, when *years of continuous democracy* is regressed on labor force in services and economic development, only GNP per capita has a significant effect.[5] Model B thus corresponds to a very different pattern of causal inference than model A. From his estimates for model A, Inglehart concluded that "economic development per se does not necessarily lead to democracy. Only insofar as it brings appropriate changes in social structure and political culture does it enhance the viability of democratic institutions" (1990, 44). By contrast, if one makes a single change in the causal ordering of the variables, a change that reflects more correctly the temporal ordering of the measurements, one then finds that economic development enhances the viability of democratic institutions directly and that social structure is irrelevant. Moreover, the effect of economic development on civic culture attitudes becomes indirect, transmitted through the influence of economic development on producing stable democracy.

As they stand, we believe that model B is more defensible than model A, but neither model addresses the possibility of a causal effect of civic culture on change in level of democracy—the question that is at the heart of the civic culture thesis. If civic culture attitudes of the general public are a relevant determinant of democratization, then high levels of civic culture should produce increases in a country's level of democracy and low levels of civic culture should inhibit the development of democracy. In other words, civic culture attitudes should have a positive effect on change in level of democracy over time.

Currently, the most widely used indicators of the extent to which a country's political system is democratic or repressive are the ratings of political rights and civil liberties on seven-point scales developed by Raymond D. Gastil for Freedom House and reported annually since 1972. The political rights and civil liberties scales are based on check-lists that cover 11 and 14 different criteria, respectively (see Gastil 1991); and they appear to have good reliability as suggested by very high correlations between them and other conceptually similar measures available for particular years only (cf. Bollen 1986, 588; Banks 1986; Coppedge and Reinicke 1991). We use the average political rights and civil liberties ranking, converted to a scale of 0–100, as an index of a country's level of democracy in a given year and then compute a mean level of democracy score over two intervals, 1972–80 and 1981–90 (see Appendix A).

The regression equations reported in Table 1 test for the possibility of an effect of civic culture attitudes

TABLE 1

Effects of Civic Culture Attitudes and Macrosocietal Variables: European-oriented Sample

INDEPENDENT VARIABLES	EQUATIONS EXPLAINING LEVEL OF DEMOCRACY, 1981–90					
	1.1	1.2	1.3	1.4	1.5[a]	1.6
Intercept	37.98	64.98	30.22	95.30	69.06	58.62
Level of democracy, 1972–80	+.60* (.17)	+.75* (.10)	+.69* (.11)	+.16 (.18)	+.34* (.17)	+.52* (.20)
Civic culture index, 1981–86[b]	+.05 (.54)	—	—	—	—	—
Level of life satisfaction, 1981–86	—	−5.16 (3.34)	—	—	—	—
% interpersonal trust, 1981–86	—	—	+.06 (.23)	—	—	—
% support revolutionary change, 1981–86	—	—	—	−2.45* (.73)	−.68 (.88)	—
ln% support revolutionary change, 1981–86	—	—	—	—	—	−6.50 (7.01)
Adjusted R^2	.58	.74	.71	.76	.54	.63
Number of cases	20	24	23	21	20	21
	1.7	1.8[c]	1.9	1.10[c]	1.11	1.12
Intercept	35.44	38.09	−11.15	−34.19	−22.06	113.52
Level of democracy, 1972–80	+.76* (.13)	+.80* (.14)	+.68* (.10)	+.64* (.10)	+.84* (.25)	+.21* (.11)
% defend society against subversives, 1981–86	−.38 (.31)	−.69 (.44)	—	—	—	—
% support gradual reform, 1981–86	—	—	+.64* (.28)	+.99* (.33)	+.61* (.29)	+.58* (.17)
Years of continuous democracy, 1900–1986	—	—	—	—	−.033 (.17)	—
% labor force in tertiary sector, 1965	—	—	—	—	+.20 (.56)	—
Gross national product per capita, 1955–60	—	—	—	—	−.0068 (.0072)	—
Top 20% income share, 1970–80	—	—	—	—	—	−1.79* (.37)
Ethnolinguistic fractionalization, 1960–65	—	—	—	—	—	−.15* (.06)
Adjusted R^2	.64	.65	.70	.74	.67	.90
Number of Cases	21	20	21	20	21	19

Note: Entries are unstandardized regression coefficients (standard errors in parentheses).
[a] South Africa excluded.
[b] Mean of life satisfaction, interpersonal trust, and opposition to revolutionary change.
[c] Norway excluded.
$^*p \leq .05$, one-tailed.

on change in democracy from the 1972–80 interval to the 1981–90 interval. The dependent variable is a country's mean annual level of democracy during 1981–90; and since a country's mean annual level of democracy during 1972–80 is included in the equation, the effects of the civic culture variables are effects on change in democracy, that is, the level of current democracy *independent* of its previous level.

The equations in the upper panel of Table 1 test for effects on change in democracy of the composite civic culture index and each of its components analyzed separately.[6] Equation 1.1 shows that the composite index of civic culture has no significant effect on change in level of democracy. (Indeed, the coefficient on the composite index is smaller than its standard error.) Thus, if one uses only the composite index of civic culture, the causal inference of model B is supported: democracy has a strong positive effect on the development of civic culture attitudes and civic culture attitudes have no effect on democracy.

When the civic culture index is unpacked, however, one finds that the components have different effects on change in democracy. Equation 1.2 shows that life satisfaction, which is conceptually distinct from Almond and Verba's notion of civic culture, has a negative effect on change in democracy. But the coefficient on life satisfaction is not sufficiently larger than its standard error to be statistically significant at the .05 level for a two-tailed test (or a for a one-tailed test), so we cannot reject the null hypothesis of no effect. Equation 1.3 shows that interpersonal trust has no significant effect on change in democracy. By contrast, Equation 1.4 shows that the percentage of the general public in favor of revolutionary change does have a significant negative effect (the expected direction) on change in democracy.

Inspection of the distribution of scores on support for revolutionary change in Inglehart's European-oriented sample reveals the presence of an extreme outlier: South Africa (see Table A-1). Support for revolutionary change in this country is at the 25% level, almost twice as great as the other relatively high scores of 14%, 13%, and 12% for Portugal, Argentina, and Mexico, respectively. South Africa also has the lowest levels of democracy for both intervals and the most negative change, so it is obviously a potentially influential case. Equation 1.5 shows that when South Africa is deleted, the coefficient on support for revolutionary change is radically reduced in size to a nonsignificant value that is smaller than its standard error. However, since there is neither a good theoretical reason for deleting South Africa nor any reason to suspect that the score of 25% is due to measurement error, deletion of South Africa is arbitrary. A preferable remedy is to reduce the influence of South Africa's extreme score by logging the scores on the revolutionary change variable. Equation 1.6 shows that across all cases in the European-oriented sample, logged (base e) revolutionary change has no significant effect on change in democracy. So one may conclude that the significant negative effect of revolutionary change in equation 1.4 is an artifact of the extreme distance between South Africa's score and the other scores.[7] Consequently, analysis of the components of the civic culture index only reinforces the inference that civic culture attitudes do not have a causal effect on democracy.

There is still one more avenue to explore, however, with the available cross-national data on civic culture attitudes. Support for revolutionary change is one of three options that respondents were asked to choose between in describing their attitude about social change (see Appendix A). The other choices were to defend the status quo against subversive forces or to improve society gradually by reforms. The problem with using support for revolutionary change as an indicator of a civic culture attitude is as follows. First, one has to ask what kind of civic culture attitude is being measured by support for revolutionary change. Although Inglehart (1988, 1990) does not address this question, it is presumably system affect: support for revolutionary change implies a strong rejection of the existing political system and a preference for achieving societal change by the use of force and violence. Since violent rebellion has hardly ever resulted in the establishment of a democratic regime and since high levels of political violence are incompatible with stable democracy support for revolutionary change clearly is an attitude that is not supportive of democratic political institutions (Huntington 1984, 213–14; Powell 1982, 168–70). But lack of support for revolutionary change does not necessarily imply support for democratic institutions. Respondents who do not favor revolutionary change may prefer the option of defending their society against subversive forces. This kind of extremely conservative attitude about societal change implies support for repression of dissent, which could inhibit a country from establishing or maintaining a high level of democracy. Thus, the expected inverse relationship between support for revolutionary change and democratization will be weakened by the fact that people who oppose revolutionary change may not necessarily support democracy. Indeed, as can be seen from the distribution of scores on the defend society against subversives option (see Table A-1), sizable minorities of them endorse this conservative option; and this kind of attitude implies support for repressive measures by the state to suppress dissent, which would hinder democratization.

The *support for gradual reform* option is the attitude about societal change that in our view is most unambiguously supportive of democratic procedures. We assume that citizens who support democratic norms and institutions will prefer the gradual improvement of society by reforms over either revolutionary change or intransigent defense of the status quo. Therefore, we think that the system affect component of the civic culture concept is measured best by the percentage of the general public that supports gradual reform. A high level of support for gradual reform among the general public necessarily implies relatively low levels of support for the radical option of revolutionary change and the conservative option of defending society; a low level of support for gradual reform necessarily implies that either or both support for revolutionary change and intransigent defense of the status quo are relatively high. Therefore, we hypothesize that support for gradual reform will have a positive effect on change in democracy and that this positive effect will be much sharper than the negative effects of support for revolutionary change and defending the status quo.

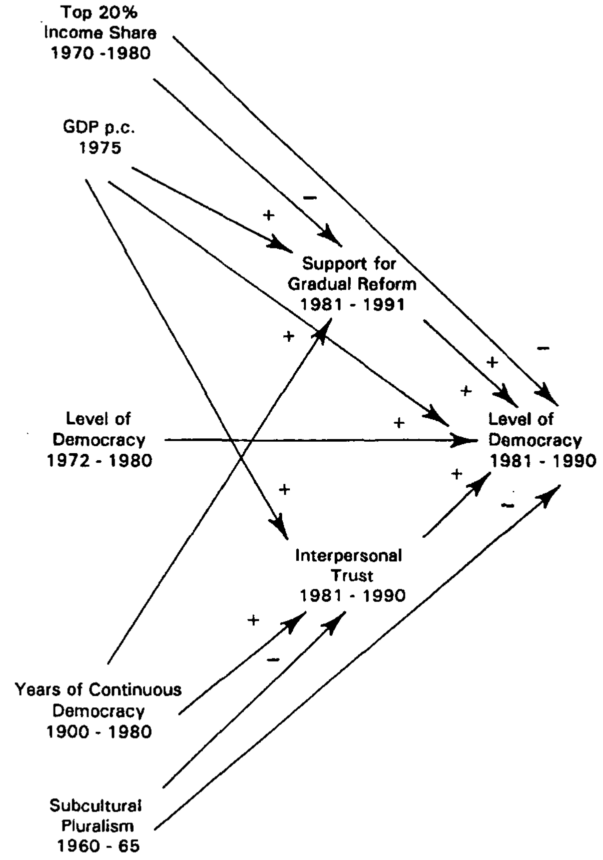
The regression equations in the lower panel of Table 1 take the other attitudes about societal change into account. We focus first on the percentage of the general public that favors defending society against subversive forces. We expect that this attitude about societal change will have a negative but relatively weak effect on change in level of democracy. Equation 1.7 shows support for our hypothesis. Norway, however, is an extreme outlier (see Table A-1). An unusually large proportion (49%) of the Norwegian general public favors defending society against subversive forces. This seems odd given the fact that only 16% of Swedes and 28% of Danes expressed support for the *defend society* option, so one might wonder if Norway's exceptionally high score is a result of measurement error. Therefore, equation 1.8 reestimates equation 1.7, with Norway excluded. Although the coefficient on *defend society* with Norway deleted almost doubles in size, it still is not significant for a one-tailed test (the t-ratio is −1.57). Thus, despite the possibility of measurement error in the Norwegian case, the negative relationship between support for intransigent defense of the status quo and change in democracy remains weak and nonsignificant.

Equations 1.9–12 include support for gradual reform as a determinant of change in level of democracy. In equation 1.9, the coefficient on support for gradual reform is positive and statistically significant, which supports our hypothesis. Although inspection of the distribution of scores on support for gradual reform does not reveal any extreme outliers (see Table A-1), Norway has the lowest score (49%). The relatively low level of support for gradual reform in Norway is due to the very large proportion of Norwegians who favor defending the status quo against subversive forces. If this is a reflection of measurement error, then inclusion of Norway may exert a downward bias on the regression coefficient for the support for gradual reform variable. Norway is therefore excluded in equation 1.10. As expected, the coefficient on gradual reform is larger (by approximately 50%) with Norway deleted, and its t-ratio is 3.0 instead of 2.29 with Norway included. Thus, exclusion of Norway favors our hypothesis even more strongly.

In equations 1.11 and 1.12, we introduce macrosocietal variables and retain Norway, since it would be arbitrary to exclude Norway on the suspicion of measurement error without having examined the actual wording in Norwegian of the question on attitude about societal change. Equation 1.11 shows that none of the macrosocietal variables in Inglehart's model has a significant effect on change in democracy, whereas the positive effect of support for gradual reform remains significant. Equation 1.12 includes income inequality (see Appendix B) and an indicator of the concept of subcultural pluralism, the index of ethnic and linguistic fractionalization (Taylor and Hudson 1971; tbl. 4.15, col. 3; original scores expressed as fractions between 0 and 1 are multiplied by 100). These two macrosocietal variables were omitted from Inglehart's model, but they have significant negative effects on change in democracy, as expected.[8] The coefficient on support for gradual reform remains significant when income inequality and ethnolinguistic fractionalization are taken into account. The results of equations 1.11 and 1.12 thus show that the positive effect of support for gradual reform on change in democracy is not a spurious reflection of macrosocietal causes.

FIGURE 2

An Alternative Causal Model of Economic and Cultural Determinants of Democracy


Top 20% Income Share 1970 - 1980
GDP p.c. 1975
Support for Gradual Reform 1981 - 1991
Level of Democracy 1972 - 1980
Level of Democracy 1981 - 1990
Interpersonal Trust 1981 - 1990
Years of Continuous Democracy 1900 - 1980
Subcultural Pluralism 1960 - 65
+
−

An Alternative Macro–Micro Causal Model

An alternative to Inglehart's causal model of economic and cultural determinants of democracy is shown in Figure 2. The important differences between Inglehart's model (Figure 1, model A) and ours are as follows. First, Inglehart's dependent variable (years of continuous democracy) is a predetermined independent variable in our model. This avoids the untenable assumption that civic culture attitudes measured in the 1980s cause previous years of continuous democracy. Our dependent variable is change in a country's average level of democracy from 1972–80 to 1981–90, that is, level of democracy during the 1980s independent of level of democracy during the 1970s. It is plausible to assume a priori that level of civic culture attitudes in the 1980s could be a cause of change in level of democracy from the 1970s to the 1980s. And the possibility of a reciprocal causal effect of experience of democracy on civic culture attitudes is incorporated into our model by inclusion of the *years of continuous democracy* variable as an antecedent cause of civic culture attitudes.

Second, the concept of civic culture is measured only by attitude about societal change and interpersonal trust. Life satisfaction (which is neither an attitude of specifically political culture nor a component of Almond and Verba's conceptualization of civic culture) is excluded. Also, instead of combining civic culture attitudes into a composite measure, we allow for the possibility that the relationships between these variables and change in democracy might differ in magnitude and significance. Furthermore, we replace support for revolutionary change with support for gradual reform because we think that the latter attitude about societal change is a much more unambiguous indicator of the kind of system affect that is most conducive to democracy.

Third, the percentage of the labor force employed in the tertiary sector is not included in our model because (1) we are skeptical about its validity as a proxy for the size of the middle class, (2) there is no evidence from large-sample quantitative cross-national research to indicate that it is a relevant determinant of variation in level of democracy, and (3) it is irrelevant either as a predictor of years of continuous democracy in model B (see Figure 1) or as a predictor of change in level of democracy (see equation 1.11). Instead, the alternative model includes—in addition to level of economic development—income inequality and subcultural pluralism, the two macrosocietal variables that we hypothesize are relevant causes of democratization omitted from Inglehart's model (equation 1.12 supports this hypothesis). Income inequality and subcultural pluralism are predicted to have negative effects on change in level of democracy. Also, support for gradual reform is expected to be higher in countries with lower levels of income inequality, while interpersonal trust is expected to be higher in homogeneous than in heterogeneous societies. And we include the possibility that level of economic development may have a positive effect on each of these civic culture attitudes.

Fourth, we treat the macrosocietal variables that are antecedent in time to level of democracy in 1981–90 as predetermined variables. Since our sample is limited by the small number of cases with information on civic culture attitudes, it is inappropriate for addressing the question of causal relationships between the macrosocietal variables, and this question is not relevant to our concern with linkages between structural characteristics of societies, civic culture attitudes, and democracy. The macroeconomic variables—income inequality and gross domestic product (GDP) per capita—are measured for the 1970s (see Appendix A). The ethnolinguistic fractionalization indicator of subcultural pluralism is available only for 1960–65. *Years of continuous democracy* is Inglehart's index dated to 1980. This variable should be highly correlated with level of democracy over 1972–80, as well as with the predetermined macroeconomic variables; but we do not expect it to have a direct influence on change in level of democracy that is independent of the other determinants of change in level of democracy.

The hypotheses to be tested are represented by the arrows of causation between the predetermined macrosocietal variables, the civic culture attitudes, and change in level of democracy. Support for Inglehart's (1988, 1990) assumption that civic culture attitudes are a powerful unidirectional determinant of democracy will be found if support for gradual reform and/or interpersonal trust have strong effects on change in level of democracy, while long-term experience of democracy (years of continuous democracy, 1900–1980) does not have an effect on civic culture attitudes. Support for the hypothesis of reciprocal causation between civic culture and democracy, which Almond (1980) argues was the original hypothesis of *The Civic Culture* (Almond and Verba 1963), will be found if long-term experience of democracy has a positive effect on civic culture attitudes, and civic culture attitudes, in turn, have positive effects on change in level of democracy. Support for Barry's (1978) hypothesis that civic culture attitudes are primarily an effect rather than a cause of democracy will be found if long-term experience of democracy has a positive effect on civic culture attitudes and civic culture attitudes have no effect on change in level of democracy.

For the sake of simplicity, our causal model entails the assumption that there is no simultaneous causal effect of level of democracy during 1981–90 on civic culture attitudes during 1981–91. The validity of this assumption can be tested, however. If we find evidence of an effect of a civic culture attitude on change in democracy, then we will test for the possibility that the effect on change is inflated by simultaneity bias.

Determinants of Change in Level of Democracy

During the 1980s democracy expanded dramatically in the world. Many countries either inaugurated a democratic regime for the first time or else reestablished democratic rule after a previous lapse, and countries that had previously established stable democratic rule maintained it. Our data set—limited as it is mainly to European, North American, and Latin American countries—nevertheless reflects the global trend of the decade (see Appendix A). Four countries in our sample (Argentina, Greece, Spain, and Portugal) established stable democracy during the 1980s. In this group, the mean level of democracy during the 1970s had ranged from 42 to 68, and the mean level of democracy during the 1980s increased to a range of between 79 and 92. A fifth country, Honduras, came close to reaching the upper quarter of the democracy range (75 to 100), as its mean score changed from 43 over the 1970s to 73 over the 1980s. In addition to these instances of substantial gains in democratization, 16 countries maintained a high, stable mean level of democracy in the range of 83 to 100, with change in the range of less than ±10 points. Only 6 countries did not either achieve or maintain a relatively high level of democracy. One of them, Panama, registered a gain in mean level of democracy of more than 10 points, but in this instance the change was from a very low level of democracy during the 1970s (20) to a relatively low level during the 1980s (39). The other five countries that remained at intermediate-to-low mean levels of democracy during the 1970s and

TABLE 2

Effects of Civic Culture Attitudes and Macrosocietal Variables: Expanded Sample

INDEPENDENT VARIABLES	EQUATIONS EXPLAINING LEVEL OF DEMOCRACY, 1981–90					
	2.1	2.2	2.3	2.4[a]	2.5[b]	2.6
Intercept	−18.29	88.22	94.12	104.55	80.45	93.13
Level of democracy, 1972–80	+.57*	+.38*	+.32*	+.32*	+.32*	+.32*
	(.19)	(.17)	(.15)	(.14)	(.14)	(.13)
% support gradual reform, 1981–91	+.61*	+.63*	+.56*	+.53*	+.75*	+.62*
	(.30)	(.25)	(.26)	(.24)	(.30)	(.22)
% interpersonal trust, 1981–91	+.17	+.14	—	—	—	—
	(.25)	(.24)				
Years of continuous democracy, 1900–1980	−.07	−.10	—	—	—	—
	(.15)	(.14)				
Top 20% income share, 1970–80	—	−1.68*	−1.54*	−1.65*	−1.56*	−1.60*
		(.52)	(.52)	(.50)	(.50)	(.39)
Gross domestic product per capita, 1975	+.0028*	−.0003	−.0006	−.0003	−.00008	—
	(.0016)	(.0015)	(.0020)	(.0014)	(.00144)	
Ethnolinguistic fractionalization, 1960–65	−.23*	−.15	−.18*	−.17*	−.15*	−.17*
	(.11)	(.10)	(.09)	(.09)	(.09)	(.08)
% labor force in services, 1977	—	—	−.03	—	—	—
			(.31)			
% protestant population, 1964	—	—	−.06	—	—	—
			(.08)			
Adjusted R^2	.77	.85	.85	.85	.86	.87
Number of cases	27	25	25	26	24	25

Note: Entries are unstandardized regression coefficients (standard errors in parentheses).
[a] Greece included.
[b] Norway excluded.
*$p \leq .05$, one-tailed.

1980s were El Salvador, Guatemala, Mexico, Nicaragua, and South Africa, where slight decreases occurred. The largest decrease was in South Africa, which declined from a mean democracy score of 34 over the 1970s to a mean of 27 over the 1980s.

We use the expanded sample of 27 countries to test the hypotheses of our alternative causal model. Inclusion of the six Central American countries substantially increases variation on the measures of democracy, since, with the exception of Costa Rica, these countries have low-to-intermediate scores on level of democracy during 1972–80 and 1981–90, and no years of continuous democracy from 1900 to 1980. Variation on the macroeconomic variables is also increased substantially, especially in regard to GDP per capita, since all of the Central American countries have lower levels of this indicator of economic development than any of the countries in Inglehart's sample.

The predictions of the alternative causal model about determinants of change in level of democracy are tested by the regression equations reported in Table 2.[9] Equation 2.1 includes all of the explanatory variables except for income inequality, which is omitted because of missing data for Greece and Luxembourg. First, in regard to the civic culture attitudes, the coefficient on support for gradual reform is positive as expected and statistically significant. By contrast, the coefficient on interpersonal trust is not significant. These results are consistent with the findings from Inglehart's smaller European-oriented sample of no effect of interpersonal trust on change in democracy (equation 1.3) but a positive effect of support for gradual reform (equations 1.9–12). Variation in the percentage of the general public with high interpersonal trust thus appears to be irrelevant for democratization; whereas democratization is facilitated by the extent to which the general public favors gradual reform.[10] Second, in regard to the macrosocietal variables, *years of continuous democracy* does not have a significant direct effect on change in level of democracy, as we expected; but the coefficients on GDP per capita and ethnolinguistic fractionalization are significant and in the expected direction. The finding of a significant positive effect of level of economic development on change in democracy for the expanded sample differs from the finding of no effect for the European-oriented sample (see equation 1.11). This difference appears to be due to the fact that the expanded sample has greater variation in level of economic development than the European-oriented sample.[11]

Equation 2.2 includes income inequality. The coefficient on this variable is negative as expected; and it is more than three times as large as its standard error (t-ratio of −3.23), indicative of a strong inverse influence of income inequality on change in level of democracy. Also, when income inequality is taken into account, the coefficient on GDP per capita is reduced effectively to zero. There is a strong negative correlation between GDP per capita and income inequality ($r = -.838$); and the results of equation 2.2 indicate that income inequality is the variable with direct causal influence on democratization, while the effect of GDP per capita is indirect through its association with income inequality. This inference appears to be valid, despite the high correlation between these variables. If multicollinearity were a serious problem, then both of the parameter estimates would be imprecise and nonsignificant because their standard errors would be high. This is clearly not the case in regard to equation 2.2, however, since the coefficient on income inequality is much higher than its standard error. Another consequence of multicollinearity (if it is a problem) is that parameter estimates are unstable in the face of slight variations in the data matrix due to addition or deletion of observations or variables. This possibility is addressed in equations 2.3–5, which also focus on other concerns.

The two obviously irrelevant variables, interpersonal trust and years of continuous democracy, are deleted in equation 2.3; and we add two omitted variables, the percentage of the labor force in service occupations and the percentage of the population that is Protestant.[12] This equation is a test for the possibility of omitted-variable bias. The labor force in services variable was excluded a priori for the reasons given in our discussion of the alternative causal model (see Figure 2). Also, although Protestantism historically has been associated positively with a country's level of democracy (see Bollen and Jackman 1985), we do not think that Protestantism is relevant for explaining change in democracy from the 1970s to the 1980s because by this time all Protestant countries were stable democracies and the Catholic Church had become quite supportive of democracy. The results of equation 2.3 support our assumptions about the irrelevance of the labor force in services and Protestantism, since these variables do not have significant effects on change in level of democracy. Moreover, inclusion of them in the equation does not alter the pattern of effects observed in equation 2.2 for the highly correlated variables, income inequality, and GDP per capita.

Equation 2.1 was estimated across the full sample of 27 cases, but equation 2.2 could be estimated for only 25 cases because Greece and Luxembourg are missing data on income inequality for circa 1970–80. Equation 2.4 addresses the question of whether the absence of these cases affects the parameter estimates. Since income distribution in Luxembourg has never been measured to our knowledge, it is not possible to include that case. But in the case of Greece it is possible to substitute a 1957 measurement of the share received by the upper quintile of households (49.5%) reported by Paukert (1973). We observe that when the circa 1960 income inequality score for Greece is included, the coefficient on income inequality is significant and the coefficient on GDP per capita is not significant, just as in equations 2.2 and 2.3. Also, support for gradual reform again has a significant positive effect on change in democracy. Thus missing data does not seem to be affecting the parameter estimates.

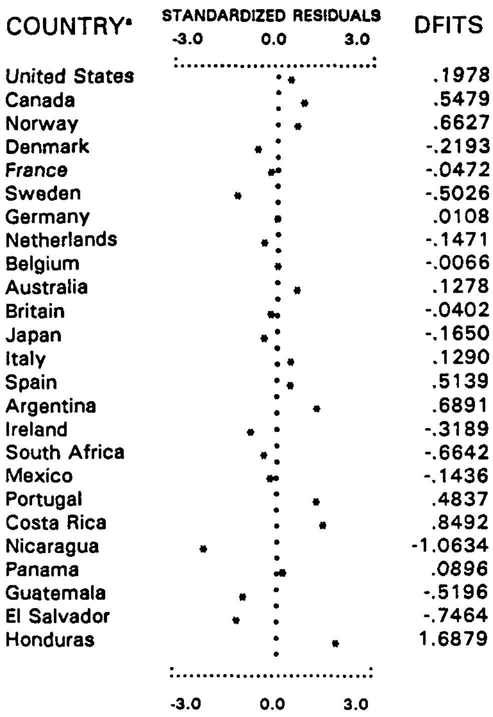
Equation 2.5 tests for the possibility that the parameter estimates could be affected by the case of Norway, where the comparatively very low score on support for gradual reform may be a reflection of measurement error. When Norway is deleted, the coefficient on gradual reform is higher than in the equations that include Norway (as expected), but the difference is relatively slight; and the other parameter estimates are quite similar. The possibility of measurement error in the Norwegian case thus has no significant influence on the results.

The irrelevant *GDP per capita* variable is deleted in equation 2.6, which is the best "trimmed" equation for predicting change in democracy from the 1970s to the 1980s. It explains 87% of the variance in change in level of democracy. The coefficients on the explanatory variables appear to be reliable estimates, since they are all more than twice their standard error and do not appear to be sensitive to multicollinearity, omitted variable bias, or measurement error. The most interesting substantive finding is the positive effect of the percentage of the general public that supports gradual reform on change in level of democracy, which supports the inference that variation in civic culture, at least as measured by this kind of attitude, may have causal influence on democratization. Now we need to address the question of whether the magnitude of this effect might be inflated by the possibility of a simultaneous reciprocal effect of level of democracy during 1980–90 on level of support for gradual reform during 1981–91. For level of democracy during 1981–90 to have an effect on level of support for gradual reform during 1981–91, a significant positive correlation must obtain between these variables. But that is not the case. The correlation between support for gradual reform during 1981–91 and level of democracy during 1981–90 is very close to zero ($r = +.058$). Therefore, the assumption that we made of no simultaneous reciprocal causation is supported.

Another important question to address in regard to the trimmed equation is the robustness of the direct effects of support for gradual reform, income inequality, and ethnolinguistic fractionalization on change in level of democracy. Given the relatively small number of cases even in the expanded sample, it is possible that the ordinary least squares (OLS) parameter estimates could be unreliable due to nonnormal residuals reflecting the presence of outliers, otherwise influential cases, and heteroscedastic distributions on the variables. Diagnostics can be used to detect the presence of specific cases that might be unusually influential. And there are alternative esti-

FIGURE 3

Casewise Plot of Standardized Residuals from Equation 2.6 and DFITS Index



Note: Countries are listed in descending order of 1975 gross domestic product per capita.

mation techniques that correct for violations of OLS assumptions.

A casewise plot of the standardized residuals from equation 2.6 is shown in Figure 3 along with each country's DFITS value. Velleman and Welsch suggest that for small samples, a case with a DFITS value greater than three times the absolute magnitude of the square root of the ratio of parameters to cases is potentially influential (1981, 236–37). This cutoff point for equation 2.6 is 1.34. The only case with a DFITS value above 1.34 is Honduras (1.69). An alternative criterion is a change in fit greater than the absolute magnitude of the standard error (1.0) of fitted values (see Moon and Dixon 1992, 208). In addition to Honduras, Nicaragua has a DFITS value (−1.06) that exceeds this criterion.[13] These cases have the highest standardized residuals (2.04 and −2.06, respectively); but they are not severe outliers and in fact are relatively well predicted, since their residuals are very close to the range of ±2. The other cases are quite well predicted by equation 2.6.

The equations reported in Table 3 compare the OLS results for 2.6 (col. 1) with a variety of alternative estimation methods. First, we calculate the coefficients and their standard errors by bootstrapping, a nonparametric method of estimation that does not depend on the distributional assumptions of OLS (see e.g., Dietz, Frey, and Kaloff 1987; Golden 1993). The bootstrapped estimates derived from 200 replications are reported in equation 3.1. They are either identical or very similar to the OLS estimates. Second, we adjust for the possibility of heteroscedasticity by using a heteroscedasticity-consistent covariance matrix to calculate standard errors of the OLS regression coefficients (see Moon and Dixon 1992). The standard errors calculated by this method are reported in equation 3.2. They are quite similar to the OLS standard errors. Third, we use the *least absolute error* method for calculating "robust" estimators that are insensitive to nonnormal residuals (see, e.g., Dietz, Frey, and Kaloff 1987; Moon and Dixon 1992). The least absolute error estimators are reported in equation 3.3. Here we observe that the coefficients on

TABLE 3

Tests of Robustness of the Coefficients for the Trimmed Equation 2.6

	EQUATIONS EXPLAINING LEVEL OF DEMOCRACY, 1981–90				
INDEPENDENT VARIABLES	2.6	3.1^a	3.2^b	3.3^c	3.4^d
Intercept	93.13	90.97	93.20	110.93	126.67
Level of democracy, 1972–80	+.32*	+.32*	+.32*	+.29*	+.25*
	(.13)	(.13)	(.11)	(.06)	(.12)
% support gradual reform, 1981–91	+.62*	+.65*	+.62*	+.42*	+.51*
	(.22)	(.21)	(.17)	(.10)	(.20)
Top 20% income share, 1970–80	−1.60*	−1.60*	−1.60*	−1.67*	−1.99*
	(.39)	(.41)	(.37)	(.18)	(.36)
Ethnolinguistic fractionalization, 1960–65	−.17*	−.17*	−.17*	−.13*	−.21*
	(.08)	(.08)	(.06)	(.04)	(.07)

Note: Entries are unstandardized regression coefficients (standard errors in parentheses). N = 25.
[a] Ordinary least squares with bootstrapped estimates (200 replications).
[b] Heteroscedasticity-consistent covariance matrix.
[c] Least absolute error estimators.
[d] Bounded influence estimators.
*$p \leq .05$, one-tailed.

TABLE 4

Regressions of Civic Culture Attitudes: Expanded Sample, 1981–91

	EQUATIONS EXPLAINING % SUPPORT GRADUAL REFORM			EQUATIONS EXPLAINING % INTERPERSONAL TRUST		
INDEPENDENT VARIABLES	4.1	4.2	4.3[a]	4.4	4.5	4.6
Intercept	72.30	83.0	86.51	44.56	29.51	32.07
Level of democracy, 1972–80	+.09 (.14)	+.09 (.15)	+.03 (.13)	−.09 (.16)	−.13 (.16)	—
Years of continuous democracy, 1900–80	+.02 (.10)	−.02 (.10)	+.03 (.09)	+.28* (.11)	+.35* (.11)	+.18* (.05)
Top 20% income share, 1970–80	—	−.17 (.47)	−.17 (.40)	—	+.27 (.51)	—
Gross domestic product per capita, 1975	−.0016 (.0011)	−.0019 (.0013)	−.0015 (.0011)	−.0014 (.0012)	−.0007 (.0014)	—
Ethnolinguistic fractionalization, 1960–65	+.04 (.08)	+.04 (.09)	−.01 (.07)	−.07 (.09)	−.09 (.09)	—
Adjusted R^2	−.04	−.08	−.11	.20	.31	.31
Number of Cases	27	25	24	27	25	25

Note: Entries are unstandardized regression coefficients (standard errors in parentheses).
[a] Norway excluded.
$p \leq .05$, one-tailed.

support for gradual reform and ethnolinguistic fractionalization are reduced somewhat; but since these coefficients are still much larger than their standard errors, the differences are inconsequential substantively. Fourth, we use bounded influence estimators that reduce the influence of cases with high DFFITS values (see Moon and Dixon 1992). The bounded influence estimators reported in equation 3.4 are slightly smaller than the OLS estimators for support for gradual reform and ethnolinguistic fractionalization, and they are somewhat higher for income inequality and ethnolinguistic fractionalization. But again there are no substantive differences. In sum, the results of equations 3.1–4 indicate that the OLS parameter estimates of equation 2.6 are robust.

Determinants of Civic Culture Attitudes

We now turn to the question of the influence of democracy on civic culture attitudes. The predictions of the alternative causal model about determinants of civic culture attitudes are tested by the regression equations reported in Table 4. These equations assume that there are no simultaneous causal relationships between support for gradual reform and interpersonal trust. That assumption is supported by the fact that these variables are uncorrelated both in the full extended sample ($r = -.153$, $N = 27$) and in the extended sample of 25 cases with information on all variables ($r = -.122$).

The first prediction equation for support for gradual reform (equation 4.1) is estimated across all 27 cases and excludes income inequality because of missing data. None of the macrosocietal variables have significant effects on support for gradual reform. This is because there are no significant bivariate correlations between any of the macrosocietal variables and support for gradual reform.[14] Equation 4.2 shows that the macrosocietal variables also do not have significant effects on support for gradual reform when income inequality is included as a predictor. Again, this reflects the absence of significant bivariate association between the macrosocietal variables and support for gradual reform.[15] Equation 4.3 excludes Norway due to the possibility of measurement error, but deletion of Norway has no appreciable effect on the results.

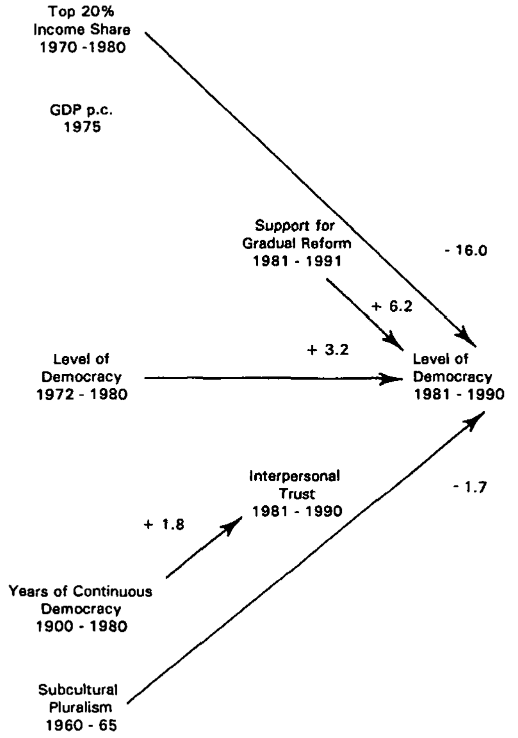
Interpersonal trust, by contrast, is correlated significantly and positively with level of democracy (1972–80) and years of continuous democracy (1900–80) in the full extended sample;[16] and it is correlated significantly and positively with these variables and significantly and negatively with income inequality in the extended sample of 25 cases.[17] Equations 4.4 and 4.5 show that in the multivariate context the only significant predictor of interpersonal trust is years of continuous democracy. Equation 4.6 is the trimmed equation (nonsignificant variables deleted) for interpersonal trust estimated across the sample with information on all the variables in the causal model.

Estimates of Causal Impact

Figure 4 shows the estimates of causal impact for our causal model of relationships between macrosocietal structural variables, civic culture attitudes of the general public, and democracy.[18] The results of testing this model of change in level of democracy do not support the argument that civic culture attitudes are the most important determinant of democratization.

FIGURE 4

Coefficients for the Alternative Causal Model of Economic and Cultural Determinants of Democracy


Top 20% Income Share 1970 -1980
GDP p.c. 1975
Support for Gradual Reform 1981 - 1991
- 16.0
+ 6.2
+ 3.2
Level of Democracy 1972 - 1980
Level of Democracy 1981 - 1990
Interpersonal Trust 1981 - 1990
- 1.7
+ 1.8
Years of Continuous Democracy 1900 - 1980
Subcultural Pluralism 1960 - 65

Note: The coefficients are the ordinary least squares regression coefficients for equations with variables having nonsignificant coefficients deleted, where each coefficient is multiplied by 10 to show the impact of a change of 10 units in the independent variable on the dependent variable.

For this sample, the most important determinant is income inequality, which has a strong negative effect on change in level of democracy; that is, an increase of 10 percentage points in the size of the upper-quintile income share is associated with a decrease of 16 points in level of democracy. The other macroeconomic variable—level of economic development as measured by GDP per capita—has no direct effect on change in level of democracy. Since there is a strong inverse correlation between GDP per capita and the size of the upper quintile income share, economic development is relevant to democratization indirectly, through its association with income inequality. The other structural variable that is directly relevant to democratization—ethnolinguistic fractionalization—has a small negative effect on change in level of democracy; that is, an increase of 10 points in a country's level of ethnolinguistic fractionalization is associated with a decrease of only 1.7 points in its level of democracy.

Our argument about the importance of analyzing the two civic culture attitudes separately is strongly supported. The percentage of the general public that supports gradual reform is empirically unrelated to the percentage with high interpersonal trust; and these variables also have very different relationships with democracy. Support for gradual reform has a positive effect on change in level of democracy (i.e., an increase of 10 percentage points in the general public supporting gradual reform is associated with an increase of 6.2 points in a country's level of democracy) and is unrelated to long-term experience of democracy. The findings regarding support for gradual reform thus support the hypothesis of a unidirectional effect of civic culture on democracy. By contrast, the findings regarding interpersonal trust support the hypothesis of a unidirectional effect of democracy on civic culture. This is because interpersonal trust is unrelated to change in level of democracy, but long-term experience of democracy has a positive effect on interpersonal trust; that is, an increase of 10 years of continuous democracy is associated with an increase of 1.8 percentage points in the proportion of the general public with high interpersonal trust.

CONCLUSION

We addressed the question of the relevance of attitudes of the general public for the establishment of stable democratic regimes by testing a causal model of macrosocietal and microattitudinal determinants of change in level of democracy. The model was designed to enable us to draw inferences about the possibility of unidirectional or reciprocal causation between civic culture attitudes and democracy. Although our sample of countries for testing the model is restricted to a maximum of 27 cases with data on civic culture attitudes, it nevertheless contains reasonably large variation on change in democracy that corresponds to the global trend of the 1970s and 1980s. So, despite its limitations, our data set affords an opportunity to go beyond speculation and actually test hypotheses about the relative importance of civic culture attitudes for democratization.

Interpersonal trust is one kind of civic culture attitude that has been assumed by many scholars to be an important attitudinal prerequisite of the establishment of stable democracy (e.g., Almond and Verba 1963; Dahl 1971; Inglehart 1988, 1990). However, we found that variation in the percentage of the general public with a high level of interpersonal trust is unrelated to change in a country's level of democracy. Low levels of interpersonal trust thus do not appear to be an impediment to democratization: Argentina, Portugal, and Spain registered substantial increases in level of democracy from the 1970s to the 1980s despite relatively low interpersonal trust levels of 21%, 28%, and 35%, respectively; and Belgium, France, and Italy were able to maintain high levels of stable democracy despite low interpersonal trust levels of 29%, 26%, and 27%, respectively. Relatively high levels of interpersonal trust also do not necessarily promote democratization. Neither Guatemala nor Panama were able to move above intermediate levels of democracy despite above-average interpersonal trust levels of 40% and 43%, respectively. (The average level of interpersonal trust for all 27 countries was 39%.)

Interpersonal trust is not unrelated to democracy, however. A country's long-term experience of democracy (as measured by its years of continuous democracy since 1900) is estimated to have a positive effect of moderate magnitude on the percentage of the general public with a high level of interpersonal trust. In our view, this is not surprising. Democracy works in practice through the peaceful collective action of groups of citizens; and because peaceful collective action is grounded in a spirit of cooperation, the institutional opportunities for peaceful collective action afforded by democratic regimes could be expected to promote relatively high levels of interpersonal trust.

Although interpersonal trust appears to be a product of democracy rather than a cause of it, there is one kind of attitude that supports the civic culture thesis. This is the percentage of the general public that prefers to change society by gradual reform instead of changing it by revolutionary action or defending it staunchly against subversive forces. Support for gradual reform has a positive effect on change in level of democracy, and it is unrelated to long-term experience of democracy. Thus the strong assumption made by Inglehart (1988, 1990) of a unidirectional causal effect of civic culture on democracy is supported in this instance.

There is an interesting implication of the fact that support for gradual reform not only is not a function of prior experience of democracy but also is not associated with any of the other structural variables in our causal model. The level of support for gradual reform in a country is independent of (and so is not constrained by) its level of economic development, the extent to which income is distributed unequally, and the subcultural heterogeneity of its population. Therefore, a high level of support for gradual reform could compensate for the presence of levels of these structural variables that are not conducive to democratization. A case in point is Honduras. Honduras has the lowest level of economic development of all the countries in our sample, and it has the highest level of income inequality. But in Honduras, 85% of the general public prefers gradual reform to revolutionary change or defending the status quo, which is the highest level of support for gradual reform of all the countries in our sample. It is possible that the very high level of support for gradual reform among the Honduran general public could have had a strong or even decisive influence on the transition from authoritarian rule to democracy in this country whose prospect for democracy from the perspective of macroeconomic preconditions was quite poor.

Although support for gradual reform appears to be an important civic culture attitude because it can promote democratization even in countries that lack conducive structural properties, nevertheless, in evaluating the civic culture theory one must keep the following caveats in mind. First, the magnitude of the positive effect of support for gradual reform on change in democracy is relatively small in comparison with the magnitude of the negative effect of income inequality. Second, the other variables included by Inglehart under the rubric of civic culture do not have statistically significant effects on change in democracy.[19] Thus, overall, the results of our analysis of causal linkages between levels of civic culture attitudes and change in level of democracy are not supportive of the thesis that civic culture attitudes are the principal or even a major cause of democracy.

Our conclusion about the causal relevance of civic culture attitudes for democracy is thus at odds with Inglehart's (1988, 1990) conclusion. Since we are confident that our findings are robust statistically, we doubt that relationships more supportive of the civic culture thesis will be found with larger and more geographically diverse samples. However, it is certainly possible that the single-item indicators of civic culture attitudes used by us and by Inglehart are too crude and that stronger causal relationships might be found with potentially more reliable multiitem indexes.

The single most important explanatory variable in our causal model of determinants of democratization is not an attitude of the general public but rather a macroeconomic variable—income inequality. The size of the income share of the richest quintile of households in a country was found to have a relatively strong negative effect on change in level of democracy from the 1970s to the 1980s. The transitions from authoritarian rule to stable democracy (so far) in Argentina, Portugal, and Spain appear to have been facilitated by the existence of relatively egalitarian distributions of income (upper-quintile income shares in the range of 41% to 50%). By contrast, the continuation of relatively low levels of democracy during the 1970s and 1980s in El Salvador, Guatemala, Mexico, Nicaragua, Panama, and South Africa has gone hand-in-hand with extremely inegalitarian distributions of income (upper-quintile income shares in the range of 57% to 61%). We think that there is a compelling causal logic for why high levels of income inequality would make it difficult to sustain a high mean level of democracy for very long. High levels of income inequality are likely to produce either a high level of rebellious political conflict (Muller and Seligson 1987) or else the perception among elites of a threat of rebellious political conflict and lower-class revolution. Therefore, executive or military coups to quell mass rebellion and preserve elite privileges are likely to occur in countries with inegalitarian distributions of income that attempt to establish democracy.[20]

In future macro-micro research designs a useful new direction to take may be to shift the focus at the micro level from attitudes of the general public to attitudes of elites. Since elites have greater opportunity and ability than the general public to influence the kind of regime a country will have, their attitudes should be given special emphasis in political culture models. Dahl (1971) attributes great importance to the attitudes of political activists and leaders as a cause of the establishment and stability of democracy. Higley and Burton (1989) make the even stronger argument that the single critical determinant of the stability of democratic regimes is consensus among elites in general on support for democratic institutions and values.

Empirical support for an emphasis on elite attitudes is provided by the pioneering study of Gibson (1988), which is limited however to secondary analysis of relatively old data collected in the mid-1950s for only a single country, the United States. Gibson tested a causal model of relationships between attitudes of political tolerance in the general public, political tolerance of elites, and repressive political legislation of state governments. He found statistical support for the inference that variation in the extent of repressive legislation against communists during the late 1940s and 1950s was an inverse function of the level of elite political intolerance and that the level of political intolerance among the general public was not directly relevant (although it was highly correlated with the level of elite intolerance). In the context of the causal model tested here, an important open question is whether change in the repressiveness of the political system, as measured by change in level of democracy, is affected by the level of support for gradual reform among the general public when variation in elite support for gradual reform is taken into account.[21] Thus, a final caveat in regard to the civic culture thesis is that we do not know whether the apparent positive influence on change in democracy of support for gradual reform among the general public is a genuine effect or is a spurious reflection of association between general public attitudes and elite attitudes, which might be the operative causal variable.

APPENDIX A: DATA

Scores on the democracy, macroeconomic, and civic culture variables are reported in Table A-1 for the 27 countries with information on attitudes about social change and interpersonal trust. The countries are listed in descending order of their level of gross domestic product per capita in 1975. The six Central American countries all have gross domestic product per capita scores below that of Portugal, the poorest country in Inglehart's European-oriented sample,

TABLE A-1

Scores on Level of Democracy, Macroeconomic Variables, and Civil Culture Attitudes

Country	Level of Democracy, 1981–90	Level of Democracy, 1972–80	Top 20% Income Share, 1970–80	GDP per Capita, 1975	% Support Revolutionary Change, 1981–91	% for Gradual Reform, 1981–91	% Defend Society against Subversives, 1981–91	% Interpersonal Trust, 1981–91
European-oriented sample								
U.S.A.	100.00	100.00	41.4	10197	5.0	73.0	22.0	41.0
Canada	100.00	100.00	40.5	10147	5.0	74.0	21.0	49.0
Luxembourg	100.00	95.56	—	8949	5.0	70.0	25.0	32.0
Norway	100.00	100.00	37.8	8580	2.0	49.0	49.0	60.0
Denmark	100.00	100.00	38.6	8374	3.0	69.0	28.0	52.5
France	91.67	93.67	43.9	8358	9.0	72.0	19.0	25.5
Sweden	100.00	99.11	39.4	8351	4.0	80.0	16.0	57.0
Germany	92.59	97.22	42.6	8067	3.0	59.0	38.0	32.0
Netherlands	100.00	100.00	39.6	8031	3.0	71.0	26.0	45.0
Belgium	100.00	100.00	37.9	7917	7.0	72.0	21.0	29.0
Australia	100.00	100.00	43.0	7739	4.0	73.0	23.0	48.5
Britain	100.00	100.00	39.6	7082	5.0	72.0	23.0	43.0
Japan	100.00	92.81	39.3	6518	3.0	71.0	26.0	42.5
Italy	98.15	90.04	45.2	5685	8.0	73.0	19.0	27.0
Spain	91.67	53.59	41.1	5478	8.0	82.0	10.0	35.0
Argentina	78.70	41.78	50.3	4214	13.0	77.0	10.0	21.0
Ireland	100.00	96.44	39.4	4157	4.0	76.0	20.0	42.0
S. Africa	26.85	34.44	58.0	4095	25.0	54.0	21.0	29.0
Greece	87.04	68.44	—	3770	9.0	63.0	28.0	50.0
Mexico	53.70	55.44	60.9	3586	12.0	77.0	11.0	18.0
Portugal	90.74	62.89	49.1	3057	14.0	74.0	12.0	28.0
Central America								
Costa Rica	100.00	100.00	49.8	2646	.5	71.0	28.5	45.1
Nicaragua	35.19	38.89	59.0	2461	10.1	72.7	17.2	31.9
Panama	38.98	20.19	60.8	2417	7.8	65.0	27.2	43.4
Guatemala	50.93	57.26	56.5	1717	3.1	78.5	18.4	39.9
El Salvador	56.48	63.04	58.7	1482	3.3	82.3	14.4	35.4
Honduras	73.15	42.74	63.5	895	.9	85.0	14.1	50.4

which dramatically increases the variability on gross domestic product per capita.

The measurements of mean level of democracy over 1981–90 and 1972–80 are given in columns 1 and 2. These scores are derived from annual Freedom House rankings of political rights and civil liberties, converted to a scale of 0–100 (see Freedom House publications *Freedom at Issue*, Jan./Feb. 1990 and *Freedom Review* 22, no. 1 [1990]; see also Gastil 1986, 1988).[22] The 1980s were a decade of expansion of democracy throughout the world, and our sample of primarily European and Latin American states reflects this trend. The expansion trend began in the late 1970s with transitions from authoritarian to democratic rule in Portugal and Spain. In Latin America, where almost all states were governed by authoritarian regimes in the mid-1970s, there was an extraordinary shift to democracy in the 1980s, so that by 1990 almost all Latin American states were under democratic rule or close to it. The old European and European-heritage democracies (plus Japan) in our sample maintained very high levels of democracy during the 1970s and 1980s; the Southern European states Portugal and Spain increased their level of democracy substantially from the 1970s to the 1980s; and the level of democracy in two Latin American states, Argentina and Honduras, also increased substantially. Declines in democracy of five points (rounded) or more occurred only in El Salvador, Germany, Guatemala, and South Africa.

Scores on the macroeconomic variables are listed in the third and fourth columns of Table A-1. Income inequality is measured by the size of the income share received by the upper quintile of households, averaged for circa 1970 and circa 1980.[23] Greece and Luxembourg are missing data on income inequality. Economic development is measured by real gross domestic product per capita in 1975 (Summers and Heston 1988).

Data on attitude about societal change are listed in Table A-1, columns 5–7. The percentage of respondents in each country who chose the first of three options is listed in the column 5, the percentage that chose the second option is listed in the column 6, and the percentage that chose the third option is listed in the column 7. The options were presented as follows:

> On this card are three basic kinds of attitudes concerning the kind of society we live in. Please choose the one that best describes your own opinion:
>
> 1. The entire way our society is organized must be radically changed by revolutionary action.
> 2. Our society must be gradually improved by reforms.
> 3. Our present society must be valiantly defended against all subversive forces.

Support for revolutionary change among the general public is very low in most of the countries in our sample. It is below the 10% level in 20 countries, marginally above 10% in four countries (Nicaragua, Mexico, Argentina, and Portugal), and reaches a level of 25% only in the case of South Africa. Support for gradual reform is quite high in Honduras (85%), El Salvador (82.3%), Spain (82%), and Sweden (80%); it is comparatively low in Panama (65%), Germany (59%), South Africa (54%), and Norway (49%). Support for defending the status quo against subversive forces ranges from a low of 10% in Argentina and Spain to highs of 38% and 49% in Germany and Norway, respectively.

Table A-1, column 8 is the percentage of respondents who believe that most people in their country are either fairly or very trustworthy. There is considerable cross-national variation in levels of interpersonal trust. Interpersonal trust is highest in the Scandinavian countries (Denmark, Norway, and Sweden), where the level of trust in one's compatriots ranges between 52.5% and 60%. The lowest levels of interpersonal trust are found in Argentina, France, and Mexico, where one-quarter or less of the general public think that others in their country are fairly or very trustworthy.

APPENDIX B: THE CENTRAL AMERICAN PUBLIC OPINION PROJECT

The Central American Public Opinion Project of the University of Pittsburgh[24] was designed to measure the opinion of Central Americans on a wide variety of issues and included the interpersonal trust and system affect questions used here. The samples drawn from each country were designed to be representative of the urban population.[25] The decision to focus on urban areas was made in order to avoid the high cost of interviewing in rural areas, which, in these countries, are often not easily accessible.

Country samples were of area probability design. In each country, the most recent population census data were used. Within each stratum, census maps were used to select, at random, an appropriate number of political subdivisions (e.g., districts), and within each subdivision, the census maps were used to select an appropriate number of segments from which to draw the interviews.[26] Within the household, all voting-age residents were eligible for selection, and one was chosen at random (using either the "next birthday system" or a sex/age quota system).

Costa Rica was designated as the country for the pilot test of the survey items. That sample was gathered in fall 1990. The surveys in the other five countries were then conducted during the summer of 1991 and the winter of 1991–92. The design called for samples of at least 500 up to a maximum of 1,000 respondents from each country. The sample sizes for each country are as follows: Guatemala, 904; El Salvador, 910; Honduras, 566; Nicaragua, 704; Costa Rica, 597; and Panama, 500.

Notes

An earlier version of this paper was presented at the 1993 meeting of the Midwest Political Science Association, Chicago, and as a public lecture in Managua, Nicaragua that same year sponsored by the Centro de Educacion para la Democracia. We are grateful for the helpful comments that we received at these presentations. We also would like to thank Hortense Dicker, coordinator of the Nicaragua Education for Democracy Project of the American Federation of Teachers, for her assistance in arranging the lecture in Managua.

1. In responding to Barry's counterhypothesis, Almond emphasizes that in *The Civic Culture* the relationship between civic culture and democracy was conceptualized as entailing reciprocal causation: "political culture is treated as both an independent and a dependent variable, as causing structure and as being caused by it" (1980, 29).

2. Inglehart's article reports the number of cases as 24 (1988, 1219); but his book reports the number of cases as 22 (1990, 45). From the data reported in the book we find only 20 countries with scores on all three components of civic culture. These are the first 21 countries listed in Table A-1, except for Mexico (which is missing information on life satisfaction). We constructed the composite civic culture index by (1) subtracting scores on the support for revolutionary change variable from 100 so that scores are the percentage who oppose revolutionary change, (2) multiplying life satisfaction scores (originally on a 0–10 scale) by 10 so that the range of the scale is the same as for opposition to revolutionary change and interpersonal trust (0–100), and (3) computing the mean of the three variables. Gross national product per capita is the average for 1955 and 1960 if data are available for both years; otherwise it is either 1955 or 1960. Gross national product per capita and the percentage of the labor force in the tertiary sector in 1965 are from the data tape for the second edition of *The World Handbook of Social and Political Indicators* (Taylor and Hudson 1972).

3. Inglehart used LISREL to estimate path coefficients, whereas we use ordinary least squares regression. The path coefficients are standardized regression coefficients or beta weights.

4. The coefficient on years of continuous democracy is +.16 with a standard error of .04 (t-ratio of 3.83), the coefficient on labor force in the tertiary sector is −.16 with a standard error of .20 (t-ratio of −.81), and the coefficient on GNP per capita is +.0018 with a standard error of .0025 (t-ratio of .74). When the nonsignificant variables are trimmed from the equation, the standardized regression coefficient of +.83 for years of continuous democracy is the same as the bivariate correlation between years of continuous democracy and civic culture.

5. The coefficient on labor force in the tertiary sector is 1.40 with a standard error of 1.08 (t-ratio of 1.30) and the coefficient on GNP per capita is +.024 with a standard error of .013 (t-ratio of 1.85). The coefficient on GNP per capita is significant at the .05 level for a one-tailed test.

6. The number of cases for the separate components of the civic culture index is greater than 20 because an additional five countries have information on one or two (but not all three) of the components. Austria and Switzerland have information on life satisfaction only, Finland and Hungary have information on life satisfaction and interpersonal trust, and Mexico has information on interpersonal trust and support for revolutionary change. Thus the number of cases is 24 for life satisfaction, 23 for interpersonal trust, and 21 for support for revolutionary change.

7. South Africa's logged support for revolutionary change score is 3.22, and the other countries with support for revolutionary change of 10% or more have logged scores of 2.64 (Portugal), 2.56 (Argentina), and 2.48 (Mexico). The negative effect of support for revolutionary change washes out when logged scores are used because in contrast to the decline in level of democracy experienced by South Africa, the other countries with high logged scores that are close to that of South Africa experienced either substantial increases in democracy (Portugal and Argentina) or little change (Mexico).

8. The number of cases is 19 because Greece and Luxembourg are missing data on income inequality.

9. Since level of democracy 1972–80 is included in these equations, the effects of the other independent variables on level of democracy 1981–90 are effects on change in democracy (i.e., the level of current democracy *independent* of its previous level).

10. We also tested for the possibility that support for revolutionary change might have a negative effect on change in level of democracy. If support for gradual reform is replaced by support for revolutionary change in equation 2.1, the coefficient on support for revolutionary change is −.27, and its standard error is .76; support for revolutionary change thus has no significant effect on change in democracy in the larger sample, even with the extreme score of South Africa included. Alternatively, if logged support for revolutionary change is used, the coefficient on this variable is extremely small relative to its standard error (−.22 and 6.60, respectively). These findings for the larger sample reinforce our conclusion from the results of equations 1.5 and 1.6 that variation in the level of support for revolutionary change has no significant effect on change in level of democracy; and they thus support the assumption of our alternative causal model that the attitude about social change relevant for democratization is support for gradual reform rather than support for revolutionary change.

11. If equation 2.1 is estimated only for the 21 cases in the European-oriented sample, the coefficient on GDP per capita in 1975 is not significant (.05 level, one-tailed test), whereas the coefficients on support for gradual reform and ethnolinguistic fractionalization remain significant. Therefore, it must be the difference in variation of GDP per capita between the two samples that is responsible for the different effects instead of differences in model specification or in the indicator of level of economic development (GDP vs. GNP) or its timing (1975 vs. 1955–60).

12. Data on the percentage of the labor force in services for 1977 is from Taylor and Jodice 1983 vol. 1, tbl. 6.3. Data on the percentage of the population that is Protestant circa 1964 is from Taylor and Hudson 1972, tbl. 4.16.

13. If equation 2.6 is reestimated with Honduras deleted, adjusted R-squared increases to .90 and all of the coefficients on the explanatory variables remain significant at the .05 level (one-tailed) and are greater than twice their standard error. If Nicaragua is deleted, adjusted R-squared is approximately the same (.88), and the explanatory variables again remain significant and greater than twice their standard error. The only notable consequence of deleting Honduras is that the coefficient on income inequality becomes 5, instead of 4 times its standard error, while the only notable consequence of deleting Nicaragua is that the coefficient on ethnolinguistic fractionalization becomes 3 instead of 2 times its standard error. Thus these cases do not appear to be substantively influential.

14. The correlations for N = 27 between support for gradual reform and level of democracy, 1972–80; years of continuous democracy, 1900–1980; GDP per capita 1975; and ethnolinguistic fractionalization 1960–65 are −.116, −.150, −.283, and +.007, respectively.

15. The correlations for N = 25 between support for gradual reform and level of democracy, 1972–80; years of continuous democracy, 1900–1980; top 20% income share; GDP per capita 1975; and ethnolinguistic fractionalization 1960–65 are −.129, −.189, +.191, −.320, and −.025, respectively.

16. The correlations for N = 27 between interpersonal trust and level of democracy, 1972–80; years of continuous democracy, 1900–1980; GDP per capita, 1975; and ethnolinguistic fractionalization, 1960–65 are +.351, +.480, +.222, and −.196, respectively.

17. The correlations for N = 25 between interpersonal trust and level of democracy, 1972–80; years of continuous democracy, 1900–1980; top 20% income share; GDP per capita 1975; and ethnolinguistic fractionalization, 1960–65 are +.397, +.582, −.347, +.294, and −.188, respectively.

18. Since the determinants of level of democracy, 1981–90, are measured by scales that range theoretically from zero to one hundred, their relative importance can be compared using the OLS regression coefficients from equation 2.6. Since none of the predetermined macrosocietal variables are correlated significantly with support for gradual reform, no impact coefficients for them are shown. The impact coefficient for the

effect of years of continuous democracy on interpersonal trust is the OLS regression coefficient from equation 4.6.

19. In addition to interpersonal trust, life satisfaction (analyzed for the European-oriented sample only) and support for revolutionary change (analyzed for the European-oriented sample and the extended sample) were found to be irrelevant to change in level of democracy.

20. Muller (1988) found a very strong relationship between income inequality and the likelihood of a country maintaining stable democracy during 1961–80 for a sample of 33 countries but no significant relationship between income inequality and the likelihood of a country inaugurating democracy during 1945–61 for a sample of 27 countries. In regard to the latter sample, only four of these countries had relatively egalitarian distributions of income (an upper quintile share of 50% or less). Three of them did inaugurate democracy (Italy, Malaysia, and West Germany) and one did not (Chad). By contrast, among the 13 countries with very inegalitarian distributions of income (an upper quintile share of 55% or more), six did not inaugurate democracy (Colombia, Gabon, Iraq, Madagascar, Mexico, and Senegal) but seven did inaugurate it (Brazil, Costa Rica, Guatemala, Jamaica, Lebanon, Panama, and Peru). The absence of a relationship between income inequality and the likelihood of inaugurating democracy is therefore due primarily to the fact that a majority of the countries with very inegalitarian distributions of income nevertheless did attempt to establish democratic regimes. Most of them were unsuccessful, however, as breakdowns of democracy due to military coup and/or civil war occurred in Brazil, Lebanon, Guatemala, Panama, and Peru. Thus the inauguration of democracy may be facilitated by relatively low levels of income inequality, although there can be exceptions—perhaps especially if a country is very poor, like Chad. High levels of income inequality do not necessarily appear to inhibit the inauguration of democracy; however, they do make it unlikely that a country will maintain a high, stable level of democracy for very long.

21. There are several cases in our sample that suggest the possibility that the level of elite support for gradual reform might be as important as (or even more important than) the level of support for gradual reform in the general public. The countries with the lowest levels of support for gradual reform are Norway, South Africa, and Germany, where gradual reform is supported by 49%, 54%, and 59% of the general public, respectively. In the case of Norway, the comparatively very low level of support for gradual reform is due to an unusually high level of support for defending the status quo against subversive forces, which could of course reflect measurement error. Alternatively, in a stable democracy like Norway, elite support for gradual reform might be much higher than the level found in the general public. In Germany, the relatively low level of support for gradual reform among the general public also is due to a relatively high level of support for defending the status quo against subversives; and the stability of German democracy also might depend more on elite attitudes. In the case of South Africa, the very low level of support for gradual reform in the general public coincided with a low level of democracy during the 1970s that declined to an even lower level in the 1980s; but South Africa currently is in the process of attempting to establish a democratic regime. We doubt that support for gradual reform among the general public recently has undergone any sudden, dramatic increase. Rather, our guess is that there must be considerably greater support for gradual reform among South African elites than among the general public and that elite preference for gradual reform over revolutionary change or staunch defense of the status quo is the driving force behind the movement for democracy in South Africa, which also could have the effect of gradually raising the level of support for gradual reform among the general public. Thus, if we had information on elite support for gradual reform, it is possible that this variable could have an independent positive effect on change in level of democracy that might be stronger than that of support for gradual reform in the general public and consequently would provide stronger support for the role of attitudes as a determinant of democratization.

22. In *Freedom in the World,* ratings listed for years 1973–82 are for the previous year, and ratings listed for 1983 to 1987 are for the current year (Gastil 1986, tbl. 6; idem 1988, tbl. 6).

23. The sources of these data are the World Bank (Ahluwalia 1976; World Bank 1979, 1984, 1989, 1990); the United Nations Economic Commission for Latin America (Comisión Económica para América Latina 1983); the Organization for Economic Development and Cooperation (Sawyer 1976); and the International Labor Organization (Lecaillon et al. 1984)—W, U, O, and I, respectively. Data for circa 1970 encompass the interval 1964–74, and data for circa 1980 encompass the interval 1975–84. If multiple observations were available, the one for the year closest to the midpoint of the interval was used. The sources and years (with circa 1970 and circa 1980 scores in parentheses in the case of averages) are as follows for each country: Argentina, W:70 (no data for circa 1980); Australia, average of W:66–67 (38.8) and W:75–76 (47.1); Belgium, average of W:74–75 (39.8) and W:78–79 (36.0); Canada, average of W:69 (41.0) and W:81 (40.0); Costa Rica, average of U:70 (50.6) and U:80 (49.0); Denmark, W:81 (no data for circa 1970); El Salvador, average of U:70 (50.8) and U:80 (66.6); France, average of W:70 (46.9) and W:79 (40.8); Germany, average of O:70 (45.6) and W:78 (39.5); Guatemala, average of U:70 (58.8) and U:80 (54.1); Honduras, average of U:70 (67.7) and U:80 (59.3); Ireland, W:73 (no data for circa 1980); Italy, average of W:69 (46.5) and W:77 (43.9); Japan, average of W:69 (41.0) and W:79 (37.5); Mexico, average of W:69 (64.0) and W:77 (57.7); Netherlands, average of W:67 (42.9) and W:81 (36.2); Nicaragua, average of U:70 (60.0) and U:80 (58.0); Norway, average of W:70 (37.3) and W:82 (38.2); Panama, average of U:70 (61.2) and U:80 (60.3); Portugal, W:73–74 (no data for circa 1980); South Africa, I:65 (no data for circa 1980); Spain, average of W:74 (42.2) and W:80–81 (40.0); Sweden, average of W:72 (37.0) and W:81 (41.7); United Kingdom, average of O:72 (39.4) and W:79 (39.7); United States, average of W:72 (42.8) and W:80 (39.9).

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25. Official census definitions are of little help in defining urban areas, because they include population concentrations so low as barely to make them distinguishable from rural villages. For example, in some countries populations over twenty-five hundred are considered urban when, in fact, these places are at best no more than very small towns. We sought to narrow our definition of urban to include the areas of major population agglomeration. In Guatemala, this meant Guatemala City, Esquintla, Quezaltenango, and other major concentrations. In El Salvador, it meant greater metropolitan San Salvador, including the city of San Salvador (divided into 14 zones) and the eight surrounding municipios: Soyapango, Cuscatancingo, Ciudad Delgado, Mejicanos, Nueva San Salvador, San Marcos, Ilopango, and Antiguo Cuscatlán. In Honduras, it meant the nation's two large metropolitan areas, Tegucigalpa (the capital) and San Pedro Sula. In Nicaragua, this definition included Managua (the capital) and the regional cities of Leon, Granada, and Masaya. In Costa Rica, the

sample covered the greater metropolitan region, incorporating San José (the nation's capital) and the provincial capitals of the *meseta central* (Cartago, Heredia, and Alajuela). The Panama sample was confined to the metropolitan Panama City area.

26. In Central America, census bureaus divide the census maps into small areas designed to be covered by a single census taker. The maps are sufficiently detailed to show all of the dwelling units. In places like Panama City, where there are a large number of apartment buildings, lists are available that show the number of dwelling units within each building. In the larger buildings this sometimes results in more than one census segment per building. In El Salvador census maps completed two years prior to the survey were available, but the census itself had not yet been taken. The maps were not of sufficient detail to show each dwelling unit, but they did show the major streets and landmarks (e.g., churches) and did provide a housing count for each census segment.

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ON THE IDEA OF THE MORAL ECONOMY

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The moral economic school, which has flourished among anthropologists, economic historians, and classicists, has received only limited attention from political scientists. This is perplexing, since at its core is to be found an intersection of debates over rational choice theory, the character of modern and premarket societies, and the normative standing of the market—in other words, over issues of formidable importance to our discipline. I seek to correct that neglect by mapping out and critically analyzing the moral economists' conception of modernity, their critique of the economic approach to human behavior and institutions, and their attempt to formulate an Aristotelian theory of the economy. These projects, though flawed, together are more than rich enough to provide fertile ground for political scientists and philosophers. I conclude with a discussion of the moral economists' effort to develop a normative theory of the economy together with a related critique of the market.

Though familiar territory among anthropologists, economic historians, and classicists, the cluster of theories grouped under the heading *moral economy* is as yet largely uncharted terrain for political scientists and political philosophers. This is puzzling, for there is much to recommend them to our attention. They (1) offer a characterization of modernity—and of the transition from traditional societies to the present epoch—that meshes well with and fills out such provocative readings as Habermas on system and "lifeworld"; (2) are engaged in the controversy over the "economic approach to human behavior," over rational choice and related theories, and especially over the understanding of nonmarket societies;[1] (3) emerge in part from the *Gemeinschaft* critique of modernity and offer a non-Marxist critique of the liberal valuation of market society and thereby an economic extension of the communitarian/liberal dispute; and (4) can be read as an attempt to sketch the outlines of a (loosely) Aristotelian economic theory in opposition to the rights-based arguments that at present command the horizon of normative economic theorizing.[2] It is obviously not feasible on the small stage of an essay to enter into the full detail of each of these areas. I propose, therefore, to reconstruct and criticize the core elements of the moral economic approach and then develop its normative dimension more extensively. First, I sketch the ideas of the embedded and disembedded economies along their institutional, explanatory, and normative dimensions. Then I critically analyze this approach while also seeking to assess, at least provisionally, its normative heart.

THE EMBEDDED ECONOMY

The theoretical center of the moral economic approach is the defining dichotomy of the embedded economy and the disembedded, or autonomous, market. This is played out as (1) the basis of an historical account distinguishing between two grand classes of economic regime, two ways in which the economy is integrated into society; (2) the core of a higher-order debate over the proper theoretical path to be taken in understanding market and nonmarket societies; and (3) the foundation of a critique of modernity in the light of certain properties of the pre- or nonmarket world and as the framework for an alternative approach to the normative valuation of economic regimes. The idea of the embedded economy is the master concept uniting these varied facets of the theory of the moral economy, and it will serve throughout as my guiding thread.

At the most basic level, the claim that premarket economies were embedded means that the human interchange with nature was "submerged" in social relations (Polanyi 1957c, 46; idem 1960, 329; idem 1968, 65). Unfolded further, this amounts to the assertion that historically the provisioning of humans—the securing of their livelihood—was located in, or integrated through, noneconomic institutions (e.g., kinship, religion, and politics). According to Evans-Pritchard's study, *The Nuer*, for example, Nuer economic relations "always form part of direct social relationships of a general kind" (1940, 90). The specific forms of economic integration may vary (reciprocity, redistribution, exchange), but what they have in common is that they are molded, in their ends and instruments, by noneconomic forces. Where the economy is embedded, it is *Gemeinschaft*—the institutions, traditions, and norms of the community—that governs it. We could put this differently by saying that before modernity, the securing of human livelihood had no separateness—no boundary line that marked it out as distinct from the enveloping society's institutions and values (Polanyi 1977, 55; see also Polanyi 1957a, 70; idem 1957b, 250; Hopkins 1957, 299). The economy is thus limited not in its substantive signification (that humans must produce and distribute the means of their livelihood) but in its independence and so in its distinctiveness and visibility as well. We can see the sum of these propositions concerning the theoretical indistinctness of the economy—its embeddedness in direct social relations "of a general kind" and the normative gloss on those relations—in Habermas's observation that "in the nonmonetarized economic activities of archaic societies, the mechanism of exchange has so little detached itself from normative contexts that a clear separation between economic and noneconomic values is hardly possible" (1987, 163).

Consider these two illustrations of how the moral economic argument deploys the principle that before modernity much of the provisioning of human beings was embedded in noneconomic institutions and values. First the *koinōnia*, or community, encroached so heavily upon the lives of the ancient Greeks that "economic" phenomena such as investment, land acquisition, exchange, and trade were indissolubly ethical and political matters (Austin and Vidal-Naquet 1977, 113–14; Finley 1974, 32, 49; Polanyi 1977, 145–276).[3] For example, the low level of productive reinvestment in Athens is explained by the demands of full-time citizenship and by the inferior status of those engaged in business; the limited market in land by the demands of community cohesiveness; and the hostile attitude toward trade by an anticosmopolitanism born of a fear for the solidarity of the community. Seemingly economic activities were not purely such but rather were infused with the demands of status, citizenship, and so on (Austin and Vidal-Naquet 1977, 9; Humphreys 1978, 137, 143–44; Polanyi 1957a, 79; Vidal-Naquet 1965, 136; Will 1954b, 17–18).[4] The *oikos*, or household, with its noneconomic social gradations and purposes (both rooted in its sense of justice and the good) was the ideal type in the light of which the city economy was understood. The city's emporium, though something needful, was an object of suspicion and contempt, controlled and kept away from the heart of community: "Athens had need of merchants and slaves. But neither merchants nor slaves were Athens; they were an *elsewhere* of democracy, an elsewhere which made it possible" (Vidal-Naquet 1990, 15; see also Mossé 1983, 58–62; Vidal-Naquet 1981, 214).

Similar principles are applied by James Scott to the study of modern peasant villages. Scott's work focuses on the organization of these communities around the objective of securing the subsistence needs of their members. The peasant village is here said to be constituted so as to minimize risks to its members' (uncertain) subsistence. With that starting proposition, Scott's analysis could easily be cast as presenting the economic form of the village as a type of risk insurance, reciprocity and charity as devices useful for evening out the fluctuations of the family's food supply, and institutions such as the commons as a rational response to ecologically generated vulnerabilities (1976, 3, 6, 24, 29, 40–41). Yet Scott appears to reject this sort of material welfare explanation, opting instead for something closer to the moral economic argument. The economy of these villages, he says, is embedded in a moral universe in which justice (construed as the right to subsistence) is central, as is also the notion of a just price. It is this moral universe—and the location of the economy within it—that accounts for "peculiar" village practices and for the performance standards imposed by the community on its wealthy members. There is, in other words, a moral expectation based on the right to subsistence that leads to redistribution within the community. The market challenges this governing right-to-subsistence ethos, together with its norms of reciprocity and charity, and in so doing brings on a revolutionary response (ibid., 4, 42, 167, 170, 176–77, 189, 192). In short, all important aspects of the village economy—its use of land, patterns of redistribution, and ultimately its response to exogenous forces of change—are to be explained by reference to its being embedded in a noneconomic, moral universe of solidarity and the right to subsistence. From the moral economic vantage point, these otherwise disparate communities—the *oikos* and the peasant village—have in common the dominance of noneconomic norms and institutions.

I now wish to move to some of the key explanatory claims of the moral economic approach. I begin here by picking up the threads of the idea of the embedded economy. Central to that notion is the relative invisibility of the embedded economy. Invisibility means roughly that core aspects of the economy (division of labor, patterns of distribution, agent motivations and purposes) are so intermingled with noneconomic values and institutions as not to constitute a distinct theoretical sphere. More exactly, it suggests that because there are no theoretically visible boundary lines demarcating that realm from its surrounding social locus, "to speak of '*the* economy' of a primitive society is an exercise in unreality. Structurally, 'the economy' does not exist. Rather than a distinct and specialized organization, the 'economy' is something that generalized social groups and relations, notably kinship groups and relations *do*" (Sahlins 1972, 76, emphasis original; see also Finley 1974, 49; Polanyi 1957a, 71; idem 1977, 44, 55; Vidal-Naquet 1965, 136; idem 1981, 211–221). What we take to be an economic category is there "charged with a plurality of unquantifiable, incommensurable functions, among which the economic function is never isolated and constituted as such"; consequently, it has a determination that "escapes the realm of the economy" and so cannot constitute a part of an economic discourse (Bourdieu 1979, 43; Tribe 1978, 33–36).

Thus when Socrates discusses *oikonomikē* (household management, or economics) with Ischomachos (in Xenophon's *Oeconomicus*), they converse about matters such as shirking and discipline, but in a manner that makes them inseparable from ethical questions of the good life, from gender-based divisions, and from a concern with rulership and politics. The concept of the division of labor provides a good illustration of this. To be sure, labor in the ancient economy was divided, its tasks distributed, but the principles and purposes governing that allocation indicate (to the moral economist) that it cannot be considered as an "economizing" practice. Principles dictating the allocation of function typically included gender (e.g., the norm of *erga gunaikōn*, or women's work) and citizenship status, and its purpose was likely to be the creation of something more beautiful or the preservation of the appropriate order of the human community, household or city, rather than the production of more things or of things more cheaply—as in the division of labor in Adam Smith's pin factory (Finley 1982, 186–87; Hodges 1970, 216; Vernant 1969, 211) It is the institutional and value-embeddedness of the economy—the fact that it is lost in the totality of social phenomena—that renders it theoretically indistinct.

This (not, as Schumpeter maintained, the primitive and pretheoretical cast of ancient economics) explains the remarkable ethical-political quality of those writings. It invites the most basic questions of cultural interpretation: How are we to interrogate these societies about our word-concept *the economy*, which would scarcely be translatable into their idiom. Thus arises the debate with rational choice and similar approaches, for the moral economists hold that the cluster of concepts that together constitute the groundwork of modern economic theory are radically distorting when applied outside of market societies. Such applications, it is said, will produce a picture of institutions and behavior that is profoundly false, transforming persons into *homo oeconomicus* on the market model and their society into a prop for that economy (as it is in our age). What is in fact invisible will be given the illusory appearance of visibility: we will think we see in ancient Greece or in the peasant villages of the developing world something called the economy, with suitably motivated actors and with the institutions that sustain their activities. The moral economist's claim, then, is that modern economic theory, born of market society, cannot be used to analyze nonmarket communities (Dalton 1961, 1, 7; Polanyi 1966, xviii; idem 1977, 19–20). The reasons given are various but of a common stock. Scott, for example, argues that approaches that are too materialistic and individualistic, focusing on peasant budgets and goals, miss the vital component of the village economy, which is its moral vision, its sense of justice, its sense of subsistence rights, obligations to charity, and so forth. And analysts concerned with the ancient world maintain that economic concepts are singularly ill suited to understanding economic life in classical Greece because of the "heavy encroachment" there of the political with the resulting absence of an autonomous economic sphere (Finley 1965, 33; Scott 1976, 165–67, 188–89; Will 1954a, 209).

Polanyi, Sahlins, and others push these claims one step further: in so far as economic theory (and the economic approach to human behavior) rests on assumptions of economizing behavior—of scarcity-driven optimizing choices—it is for that reason, too, the product of our society, faithfully mirroring it but causing us to see nonmarket societies as if in a camera obscura (Bourdieu 1979, 1–7; Sahlins 1972, 2–39; Dalton 1961, 7; Pearson 1957, 320; Polanyi 1957b, 243–44; idem 1977). This amounts to the assertion that scarcity, surplus, and the economizing rationality that they are said to induce, the "formal" idea of the economy (and other social phenomena), are not universals but historically and culturally bounded. The range of formal theory is thereby limited to one era, the one that first gave rise to it. In other times, there may have been no conception of available choice among means, the logic of maximizing calculation may have been displaced by the demands of solidarity, or the definition of the human condition as one marked by scarcity and the endless growth of needs may have been explicitly rejected (as it was by Aristotle and is, according to Sahlins [1972, 2, 13, 37], in the "original affluent" societies). Surplus, scarcity, and economizing choices are useful analytical tools only for societies that name them as such, where they can constitute an economic discourse and that, the moral economists tell us, has not been the case for much of human history (ibid. 2, 13, 37; see also Polanyi 1957b, 243; idem 1977, 29–30). What they call the "substantive" economy—human beings provisioning themselves in an ongoing interchange with nature—converges with the "formal" conception of society and economy only in modernity.

So far, we have seen the idea of the moral, or embedded, economy as it plays out along two dimensions; *institutional* and *explanatory*. Now I also observed that the moral economic argument has a third, *normative,* side. Indeed, the very appellation under which this group of writings is gathered points directly to that ethical dimension. Reserving a full discussion of these facets, I shall for now simply mention some of the normatively oriented properties that these authors attribute to nonmarket societies. Once more the idea of the embedded economy will provide us the guiding thread, and following Polanyi, I shall privilege its (Aristotelian) "householding" variant: "[Aristotle's] famous distinction of householding proper and money-making . . . was probably the most prophetic pointer ever made in the realm of the social sciences. . . . In denouncing the principle of production for gain 'as not natural to man', as boundless and limitless, Aristotle was, in effect, aiming at the crucial point, namely the divorcedness of a separate economic motive from the social relations in which these limitations inhere" (Polanyi 1957c, 53–54). What draws the moral economist to this idea of the household is already suggestive of a normative and critical intent, for the household is seen as a nonmarket community, antiacquisitive, the "highest form of economic sociability," in which "man [is] the end of production" (Sahlins 1972, 76, 84, 86, 94; see also MacIntyre 1984, 227).

One ready way to construe that valuation is that in the embedded economy, the production and distribution of the means of human livelihood are subordinated to the pursuit of the good life broadly understood, a subordination that expresses the economy's governed character. The invisibility of the economy and of its characteristic ends is an index of this governance and of the extent to which these other goals dominate. The latter goals might include Aristotle's notion of the good life, the possibility of which presupposes the containment of provisioning activity; the bonds of community against the atomism of market society; Scott's moral universe of the peasant village's subsistence rights and corresponding sense of justice; Thompson's related observations on the English crowd; and altruism and gift economies (Mauss 1967, 66; Polanyi 1957a, 71; idem 1977, xlvii; Scott 1976, 5, 13, 33, 42, 167, 176–77, 189; Thompson 1971; Titmuss 1971). What unites them is the thesis that these humane attributes are a function of the subordination (embeddedness) of the economy in premarket societies. This estimation is sometimes qualified, as in Scott's warning against idealizing village practices and Polanyi's observations on gender-based divisions of labor and the importance of status in premodern communities. Even with these reservations, however, there is no mistaking the conclusion that for the moral economists, the move from *Gemeinschaft* to *Gesellschaft* entailed a loss of a certain vital human quality that typified earlier societies. As Evans-Pritchard observes, Mauss and others are telling us "how much we have lost, whatever we may have otherwise gained, by the substitution of a rational economic system for a system in which the exchange of goods was not a mechanical but a moral transaction" (Evans-Pritchard 1967, ix).

THE WORLD OF THE MARKET

Central to the moral economic account is that the transition from the embedded economy to market society marked a radical watershed in human history: "[We] go into *Gesellschaft* as into a foreign place." (Tönnies 1979, 4). Polanyi argues that nothing comparable to market society has ever before been witnessed. Of course, markets predate modernity, but for the most part, they were marginal, often heavily administered. In domestic markets, we still find the dominance of nonmarket institutions and relations. Thus exchanges within the community are governed by the same "general social relationships" that mold that association as a whole: the exchangers are not equal, commodity-owning ciphers but members of a particular household, male or female, young or old, and so on, with each attribute carrying a social meaning that bears upon all their transactions. Markets did not integrate society or even that part of it taken up with provisioning human beings (Dalton 1961, 9; Polanyi 1977, 7; see also Evans-Pritchard 1940, 91).[6]

Till now we have discussed only the embedded economy. In the following pages, I want to move into some of the institutional, explanatory, and normative detail of the "great transformation" that modernity is said to have wrought. At the core of this idea of a watershed change are two related claims: (1) that in modernity, the economy is disembedded, that is, it is an autonomous sphere governed by laws of its own and (2) that the disembedded economy presses outward, tending to become integrative not only of those domains concerned with our livelihood but of society as a whole. The first of these ideas is the institutional thesis, fundamental for the moral economists, of the autonomous, "self-regulating" character of markets in modernity (Hopkins 1957, 299; Polanyi 1957a, 68; Polanyi 1957c, 41, 71; Polanyi 1977, 10, 47). That, they argue, is the most striking feature of modern markets—and so it must appear, given their background theory of the embedded economy. For the first time in human history, the economy is not located in the enveloping and determining noneconomic context of the *oikos*, the city, the royal household, and so on. Institutionalized in the self-regulating market, it breaks free from the embrace of the sovereign community, becoming autonomous and therefore purely economic.

The second component of the idea of the disembedded economy is that of the market as pervasive. We saw that the moral economist holds that earlier societies were integrated through noneconomic institutions. Characteristic of modernity on the other hand is that the market threatens to become the dominant mechanism integrating the entirety of society (Polanyi 1957c, 57, 69; idem 1966, xvi; idem 1977, 9). The magnitude of the market's reach can be measured by the extent of commodification, by the range of goods subject to commercial traffic. In particular, the case for the pervasiveness of the market in modernity rests on the commodification of land and labor. When nature and persons, the world and human activity, are made into objects of sale—when that is the dominant mode for their transaction—the remaining barriers to marketization must be frail indeed. The former world is turned inside out, and now a whole society becomes embedded in the self-regulating mechanism of its own economy; the economy envelops society, refashioning its ethos and relations after its own image.

Unlike its predecessors, the disembedded economy is theoretically visible. Thus with modernity was born the concept of the economy (Polanyi 1977, 6). Before our age there was no "separateness" to much of the economy, no condition of existing apart from politics or other noneconomic institutions. Consequently, it could have no separate phenomenology. With the emergence of the disembedded economy as an autonomous sphere and with institutions, motives, and laws that are specifically economic, a lawlike and independent science of the economy construed as the "allocation of material goods to satisfy material wants" becomes possible for the first time (Jonas 1958; Polanyi 1977, 12, 47). But something else happens as well, for with market society comes the possibility of economics extending its explanatory range beyond the economy as just defined. Drawing on the "basic analytical tools" of rational maximizing behavior, markets, and prices, an economic approach to the whole of human behavior becomes feasible, one that makes visible the "implicit economic structure" of even such seemingly affectional communities as the family: monogamy is understood as "the most efficient marital form," and children as commodities "presumed to have modest price elasticities because they do not have close substitutes" (see Becker 1974, 300–301, 305; idem 1981, 106; idem 1976, 5–8, 14; Coase 1978, 207–8; idem 1988, 1–3; Posner 1983, 1, 3, 5).[7]

With society embedded in the self-regulating market, economic society and *homo oeconomicus* are not distorting overlays but rather the most adequate expression of modernity. In a mirror image of the premodern world, the economy, wrenched from its (former) embedded location, merges once again with society, now not because of its saturation by the noneconomic norms and institutions of the community but as a result of its very pervasiveness, a condition captured in the phrase "market society." Assumptions concerning the pursuit of material gain, calculation, and a certain atomism or selfishness are privileged by this approach—and rightly so, for market society does generate ever new wants, giving the impression that there never can be a state of sufficiency, and induces economizing behavior of just the sort depicted in formal theory. But the formal or economic approach goes further, on this account of it, and argues that in all times and places human beings face and act on the basis of scarcity constraints that engender economizing behavior (Dalton 1961, 4–5; Pearson 1957, 320, 330; Polanyi 1957b, 244; idem 1968, 64; idem 1977, 19–25). The pretension that these concepts are of universal applicability is, as we shall see, a major part of what the moral economist faults it for. Here—but here alone—that approach has explanatory power.

The moral economist's reflection on modernity has its institutional and theoretical dimensions, but it is powerfully motivated by evaluative issues as well. What Evans-Pritchard says of Mauss speaks also for the approach as a whole: "[He] is asking himself not only how we can understand these archaic institutions but also how an understanding of them helps us the better to understand our own, and perhaps to improve them" (1967, ix). What sort of society, the moral economist asks, would be amenable to such an explanation? A society in which the economy but not the community is self-regulating; in which contract becomes the cement of an atomized world; in which no ends other than (market) economic ones are pursued. Once more, the key notion is the disembedded economy, understood in the first instance as self-regulating and therefore purely economic. When the moral economists assert that the modern market is self-regulating and all-embracing, they mean, among other things, that it has escaped the control of the community. That opening move in the moral economist's normative argument can branch out along an array of critical avenues, including: (1) a quasi-Marxian critique of the autonomy of the market as a source of unfreedom; (2) a *Gemeinschaft*-type, communitarian sense of the loss of solidarity in market society and its replacement by an economically generated atomism; (3) a critique based on the idea of the good and its radical displacement in market modernity. Let me say a few words about each of these.

Critique 1. We saw that the notion of the self-regulating market was central both to the moral economist's description of the characteristic institutional arrangements of modernity and to the claim that only in our era is a lawlike account of strictly economic phenomena possible. More radically still, market society makes the entirety of human behaviors and institutions available to the economic approach. In earlier times, the economy was an instrument of society's purposes—the subsistence of its members or the possibility of a good life. It was a tool and, like all tools, subordinate to its users' ends. Today, conversely, it is the economy and its self-generated ends that govern and with a power such as to transform society's order and purposes and to substitute for them the logic of calculation and maximization. Not only is it extraordinarily thorough in dissolving the traditional community and its bonds, but its sway is so great that it reaches down into even the micro level, inducing choices and behavior consistent with its ends. Never has an economic system compelled individuals so universally and with such necessity (Wolf 1971, 278–79; Polanyi 1957c, 73; idem 1968, 62–63). The market, in short, extends the reach of the economy in controlling individuals, and that in turn is made possible by its disembedded, self-regulating quality. One measure of this transformation is the array of perverse effects attributed to the pervasive market, for example, effects on subsistence, labor, and the environment.[8]

Critique 2. There is a group of ideas broadly related to the *Gemeinschaft* view of modernity as the withering of community, an argument associated with the writings of, among others, Tönnies.[9] Here the critical anchor is the concept of market society as "economized," as one in which the relations and goals associated with the acquisitive economy triumph over higher ends and more human bonds, for among those things that have been lost in the great transformation is the tightly woven fabric of community, the mesh of obligations and expectations that overrode self-interest and made each person a part of a greater whole, whether as a member of a household or kinship group or as a citizen. In its stead emerged an atomized society in which the interdependency of individuals was not mediated through political, social, or religious institutions but via the market and contract. The noneconomic bonds of the world before modernity were supplanted by those forged from economic self-interest, creating thereby something very much like the community of traders, the rootless, floating world of the cash nexus, so much despised in ancient societies (Mauss 1967, 63–81; Wolf 1971, xviii, 279, 282–83; on atomism and individualism, see Polanyi 1957c, 163).

Critique 3. The contrast between economic and other (presumably superior) goods is suggested by the dominant motif of the moral economic approach, that is, the juxtaposition of economized modernity and the noneconomic premodern world. Need, fear, and acquisitiveness are the forces that here govern behavior to an extent previously unheard of: everything and everyone must be viewed as a possible object of gain or as a competitor for goods. Little room is left thereby for purposes other than those stamped in the mold of the market. The market, in other words, through its extensive presence and its capacity to structure choices, creates a situation in which *pleonexia,* the ancient malaise of the acquisitive soul, becomes a systemic property driving out goods other than its own. From that vantage point, the moral economic history of modernity can be read as a reaction to these perverse effects, as a struggle between the market and society's efforts to regain governance, to assert goods other than those of the self-regulating market. For the moral economist, modernity as market society is virtually from the moment of its birth a contested form, to be queried in its theoretical expression (the economic approach to human behavior) and challenged politically. The former we see in the work of the moral economists themselves and above all in their efforts to keep alive the memory of an economic order different from our own; the latter is evident in peasant rebellions against the encroaching market—rebellions sometimes seeking the preservation of the structures of traditional society and at other times venturing novel forms of the embedded economy (Wolf 1971, 282). It is also expressed in the emergence of both socialist politics and antimodern movements on the Right and in the creation of institutions that reassert society's governance over both the production and distribution of the means of its own livelihood and the consequences of the market for its other concerns. Such projects for reembedding the economy flow naturally from the picture of market society as ruled over by a disembedded, self-legislating economy coupled with the critical estimation of that society. They are the fitting terminus of a theory that offers us an account of nonmarket societies, not out of antiquarian or anthropological interest alone but as pointing to alternative ways in which the economy could be integrated into society. Perhaps because the idea of the moral economy has in our times been developed, for the most part, by anthropologists and others for whom the most pressing task was the *Methodenstreit,* the normative facets of their argument cannot be unfolded directly from the texts we are considering. I therefore postpone my discussion of it until the concluding section of the essay, where I draw on other writings related to this tradition.

EVALUATING THE MORAL ECONOMY

I now want to turn to an assessment of the moral economic approach, then to a reconstruction of its normative center. First, as even their critics are prepared to allow, the moral economists' most important achievements are their underscoring that markets have not always been the dominant institutional mode of economic transaction and their offering of an alternative typology of economic regimes (Hechter 1983, 173; North 1977, 706). The challenge to the moral economic school turns around the explanation of these nonmarket forms. Polanyi, we recall, argued that these societies resist analysis in terms of scarcity and an economizing rationality, a form of analysis that must inevitably dislodge their economy from its informing social nexus. This claim faces a number of competitors and counters. One maps a theory that—while rejecting an anachronistic employment of theories rooted in contemporary society—nevertheless accords primacy to economic forces broadly construed in the explanation of nonmarket societies. Marxism is one such possibility, and it employs the familiar conceptual tools of technology, scarcity, classes, states, and exploitation (see North 1981, 61–63). A second approach begins with the presence, even in market society, of institutions whose internal working are not marketlike (e.g., firm, family, government). A theory that can account for that presence may also be of use in explaining entire societies organized in a similar manner. On this view, the existence of nonmarket economies, contemporary or historical, may be understood in the light of the transaction costs associated with markets. Because of enforcement, measurement, information, and other costs, markets may, under certain conditions, simply be more expensive than nonmarket regimes. This approach is also able to provide a causal story for economic regime change as a consequence of (among other things) altered costs.[10]

And that points to one of the major inadequacies of the moral economic model, namely, that it has only thin theoretical resources with which to explain economic change—why economic regimes come into being and pass away. In particular, it is hard to see what non–ad hoc explanation would be used to account for the emergence of markets out of embedded economies. And while it certainly offers an explanation for the demise of the entirely unfettered market, it does not tell us the causal story behind the failures of the "invisible hand" (Godelier 1974, 1371–72; Hechter 1983, 183, 187; North 1977, 715). The moral economic approach is, in other words, oriented toward the static state and is therefore less valuable when the question is of change across time. That in turn awakens the suspicion that its account even of stable states may be inadequate, for if it is unable to explain their demise, it may well not have an adequate understanding of the sources of their stability either.

Let me pause here to focus on the extension of the transaction cost approach. This is a natural target for the moral economist, who might answer that in using the theory of the firm as our foundation, we are led to read back into the premodern world a sort of implicit market made up of just those kinds of institutions and motivations that were virtually absent from it, for the manager of the firm can be seen as driven by market-generated imperatives to regulate the internal workings of his organization in the most efficient manner. And because that behavior-inducing struc-

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ture is absent in nonmarket communities, theories of the firm will give a misleading picture of the foundations of the premodern rejection of markets, making them appear more economic than they in fact were. This retort offers a useful cautionary note, telling us that we must attend to the institutional specificity of the economy and to how it is understood by those for whom it is a part of their enveloping horizon. In short, it tells us that both in its being and in the type of explanation it calls for, a firm whose command-type hierarchical arrangement is shaped by market pressures is something quite different from, for example, the redistributionist kingly households of the archaic Mediterranean world or, for that matter, a modern family.

But perhaps the moral economic argument adopts too narrow a conception of the economic and economizing, which, in turn, is the result of its attempt to deny the choice-theoretic approach access to nonmarket societies by challenging the usefulness of notions of scarcity, scarcity-induced choice, and so on. To say that market-type scarcity, surplus, and the sorts of behavior that they help to structure are largely absent before modernity is, even if accurate, not to prove the case that economizing behavior does not exist there at all or that it is not an important explanatory device for understanding those societies. The economic approach may well provide an interpretative key to such societies so long as it is attentive to their defining institutional and motivational differences (Bates 1983, 139–40).[11] Scarcity, surplus, and economizing are wrongly defined if they are restricted to their market form and if that is the barrier to understanding important institutions and behaviors in nonmarket societies as economic at their root, then what is needed is to go beyond the too-pinched conception of the economic offered by the moral economists (Hechter 1983, 165n; LeClair 1962, 1180–1185; Smelser 1959, 176–77).

Indeed, when we consider core aspects of the behaviors they put forward as illustrative of the noneconomic character of these societies, we see the extent to which they lend themselves to retranslation into the vocabulary of the economic approach.[12] Take Scott's argument that peasant villages are moral economies because their governing objective is not individual maximization of wealth but the protection of the community as a whole against a collapse of subsistence. The end is "moral" and so, presumably, inaccessible to economic reasoning. Yet if we look at Scott's analysis of these villages, it becomes apparent that many of their behaviors and institutional forms are economizing responses to a situation of severe risk in regard to subsistence goods. They may well subscribe to the proposition that justice demands that all be preserved from hunger, but it is the scarcity of means to ward off this ill that produces their economizing decisions, including their resistance to the intrusions of the market with its threatening fluctuations and unacceptable distributive outcomes. Alter those scarcity constraints (through, e.g., state-led social assistance programs), and the structure of the village becomes redundant. The village, in short, is governed by risk aversion (a stance, Scott notes, not absent from capitalist corporations), and it economizes around its principal constraints. The specification of the end is, to be sure, a matter of "moral vision," of a certain perception of justice. But given that end and the scarcity situation that confronts the village, the resulting choices are amenable to economic analysis (broadly understood), as also is the study of their unintended consequences. Scott himself seems virtually to invite this sort of retranslation, saying that "peasant economics is better understood as a special case of what standard microeconomic theory would predict" (Scott 1976, 14).[13]

Alternately, we might consider that other focal point of the moral economic argument, classical Greece. Here again, the economy and its typical behaviors are arguably quite visible: scarcity and economizing are fully in evidence, though located in a nonmarket context. To see this, we might reflect on one of the most extensive ancient discussions of labor discipline and the division of labor—that set out in Xenophon's *Oeconomicus*. Socrates wants to learn how Ischomachos, a wealthy estate owner, has acquired the means to be able to engage in the affairs of his city and friends. In other words, how has Ischomachos arranged it so that he is not bound down by penury or excessive need? Ischomachos is not seeking to maximize wealth without limit but rather to be leisured, to find a way to escape from economic activity altogether. Much of their discussion centers on the managerial role of the wife in the *oikos* economy, the need for overseers, and (in an ancient version of the principal–agent problem) the issue of how to motivate the wife and overseer to do the master's work efficiently. The *Oeconomicus*, then, abounds in discussions of shirking, discipline, and motivational problems within the household that would not seem out of place in a modern text, with one exception. What leads Ischomachos to reflect carefully on the internal organization of his economy, to employ his work force most efficiently, is an ethical–political conception of the good life that requires him to maximize one very scarce good—time—for the sake of a life given over to free praxis. Scarcity, maximizing, choice of technique: all can be discerned in that distant conversation between Socrates and Ischomachos, and we should not be blinded to them by too cramped an understanding of the economic as identical with its market expression.[14]

Still, even if it seems that this issue can readily be brought to closure (as it arguably can if the moral economist's case rests solely on an excessively restrictive understanding of the economy), there nevertheless remains a concern that in this sort of translation of the idiom of the nonmarket world into our own, it may happen that "things get lost"—perhaps some very important things (Geertz 1983, 10). My tone has suggested that we could readily speak about, say, Ischomachos's maximizing rationality as expressed in the use of his wife and servants. But now turn the table and consider what Ischomachos would have answered to a query about his *oikonomikē*, his economic prowess. He might well have explained it as part of the "total social phenomena" of his household—tied it to his status as a citizen of this Athenian polis and to his being male and of a certain age. Were we to answer him, "No, we mean tell us about your *economic* skills," his response would be the same, and he would be bewildered at our pressing him to hive off whatever this "economy" is from the rest of the relations of his *oikos* and its place in the city. Or, still within the "total social phenomena" of his world (i.e., looking from within its ethical and political architecture), he might, like Odysseus take offence, thinking that we were calling him a trader, one of those marginal people, the *metics*, whose only fatherland is profit, and who therefore are the "elsewhere" of the city (*Odyssey* 8.158–68). Evans-Pritchard's Nuer would probably be similarly perplexed were we to speak about exchanges apart from (for example) kinship ties. The (arguably) jarring and counterintuitive quality of the account of the family as a community of traders intimates something of this loss, as (on a wider canvas) does Émile Benveniste's observation that "everything having to do with economic notions is bound up with much greater representations that call into play the ensemble of human relations, or of relations with the gods" (1969, 202).

Would Ischomachos' and the Nuer's perplexity be diminished if we were to inform them that what we meant by economic was a "science of human choice," of choices made by rational, self-interested maximizers—if, that is, we replaced the cramped concept of the economy by a more expansive, choice-theoretic approach? For we who live in a world suffused by calculation and by a rationality disposed toward a future as a realm of possibilities to be exploited, such a view of choice may come as naturally as the air we breathe. But in the moral economic account, that is a disposition "insensibly acquired . . . and taken for granted," which is peculiar to us and foreign to members of other civilizations for whom these specific ideas of the future, time, and rational calculation are largely absent (Bourdieu 1979, 4–8). What these ambiguities illustrate is that this part of the debate virtually calls out to be carried one step further—into fundamental questions of language and interpretation across cultures addressed to the proponents of the economic approach. Anthropologists, drawing on (or paralleling) Wittgenstein and Winch among others, have already traveled down much of this road. In thinking about the interpretation of the economy, about rational choice and related theories, we might well profit from their reflections—not necessarily from their conclusions, but at least in their questioning and challenges.

How does the idea of the premodern embedded economy provide a critical fulcrum against market society? As important as the normative valuation of nonmarket societies is to the moral economic argument, it is very difficult to get a purchase on it, because, as I have said, it is even more thinly developed in this literature than is the basis of their repudiation of market society. A promising starting point, however, is the idea of embeddedness. This notion contains a normative clause grounded in the claim that in nonmarket societies it is the community that absorbs the economy—directs it or infuses it with its values—rather than, as in modernity, being ruled by it. When we unfold the notion of the economy embedded in or under the governance of the community, what we find is the "domination moment" as central to the community and the encroachment of status and other forms of hierarchical gradation upon society and economy (Brunner 1956, 43–44; Finley 1974, 49). These aspects are not mere scarifications on an otherwise noble edifice but are crucial to the workings of many of those communities and to the shape of the economy within them. While similar properties have been observed in some peasant communities (see Bates and Curry 1992; Popkin 1979, 28, 61), I shall focus here on the concept of embeddedness in ancient Greek society.

We might begin by observing that the *oikos*, or household economy, literature was, at its birth in classical Greece, bound up with a critique of both markets and democracy for their indifference to the rank ordering of persons and of the goods that ought to be pursued (Humphreys 1983, 12). By contrast, the embedded economy was located in the *koinōnia*—in its affective bonds, in its proportional equality (hierarchy)—and was subject to its ruling ends. In this world, the economy did not disrupt the rank order of human beings but rewarded each according to his worth. Persons there are not the ciphers they will become in markets but individuals whose different worth shapes exchanges between them (Finley 1974, 34; Godelier 1986, 231). Embedded also means, in this context, that each does what is proper to his or her status—male and female, freeman and slave, adult and child. And it signified the tight solidarity of the community against the universalism of commerce (see, e.g., Aristotle, *Politics* 1258a39–1258b8; Demosthenes, *Private Orations* 37.52; Lysias, *Orations* 22.14, 31.6–7). Finally, *embedded* conveyed the sense of "subordinate to the proper ends or purposes"—not the Midas or animallike passions of the many, not the "cloak of many colors" (as Plato in *Republic* 557c mockingly calls the diversity of goods under democratic regimes) but the pursuit of *ta kala*, the beautiful life made possible by detachment from provisioning activity, a detachment secured in its turn by domination over others. Both of the two defining elements of the ancient idea of the embedded economy, as located within the framework of the community and made instrumental to the pursuit of the good life, are suffused with *hierarchy*—the good rank-ordering of ends and persons. The location of the economy—the form of its embeddedness—would have been incomprehensible to them without that cosmos. What the moral economist calls the disembedded economy of the market was condemned (as was democracy) from the standpoint of the *oikos* (and the best regime) for its egalitarianism and indifference to rank order, its neglect of the best life, and its openness to something outside of the tight bonds of the community's hierarchical solidarity.

We may suspect, then, that the moral economic reading of the embedded economy is colored by a certain romanticism (Godelier 1986, 181; on a similar temptation in communitarian political philosophy, see Kymlicka 1991, 85–90; Humphreys 1978, 38). Were this simply, in Habermas' wonderful turn of phrase, "the melancholy charm of irretrievable pasts and the radiance of nostalgic remembrance of what had been sacrificed to modernization" (1987, 329), then the moral economists' starry-eyed appreciation of the world before markets would be less troubling. But they mean to use that world as a mirror in which we may more readily see the flaws of our own, and in the course of doing that, they abstract from its particular binding cement, from its hierarchy and domination. In other words, they seem to have put to one side their insistence on sensitivity to the historical and institutional context. Yet embeddedness is hardly intelligible apart from that context, and once it is extracted from its specificity it loses much of its conceptual substance, becoming an ill-defined, if seductive, perception of a "thick" community. And as a countermodel to the market, that world sheds its lustre the moment its mode of embeddedness is unpacked. By leaving unsaid how the economy was subordinated and contained, this approach also causes us to miss something crucial about the theory of market society, namely the extent to which it too is a moral economics, developed in response to the embedded premodern household model.[15] I now want to examine critically the notion of the disembedded market and its normative implications.

As we saw, the governing juxtaposition of the moral economic argument—the embedded economy as opposed to the self-regulating juggernaut market—also yields a picture of market society ruled over by its disembedded economic institutions. There is, however, something misleading and false in this account—at least as misleading as its corollary claim that there is no recognizable economic behavior in premarket societies, for surely the market is *embedded* in both significations of that term: institutional and normative. The institutional context is evident and I shall not dwell on it here. It consists, among other things, of a mesh of property rights made operative in the form of law, of contracts and their enforcement and so on. The market, in other words, flourishes only amidst an array of "external" institutional supports.[16] When it is said that markets are institutionally disembedded, therefore, it is unclear what precisely is meant—except, perhaps, that they are not (wholly) regulated from above, as if by a very visible hand. That last way of speaking of their embeddedness permits us to see their ethical locale, for it suggests that we can think of markets as being one institutional outcome of the normative shift from status to contract. I shall develop that thought in order to suggest how the *embedded/disembedded* language may easily be seen as misconstruing the normative character of modernity.

The mode of producing and distributing the means for human sustenance embodied in the market is expressive not of a human propensity to truck, trade, and barter or of the desire to acquire ever more things but rather of a moral redrawing of the community and of the place of the economy within it. What that transition yields is a new form of moral embeddedness for the economy. The attributes claimed for it are familiar: an economy whose actors are considered equals and a system indifferent to their noneconomic attributes; a contractarian, voluntaristic institutional context for exchanges; and the view that the public authority should not decide among preferences—that one is entitled to live one's life "from the inside," selecting and ordering one's preferences according to the good as one understands it and seeking to engage the voluntary cooperation of others in one's pursuit of them. At its foundation, this redrawing involves a shift away from the patriarchal household model, with its hierarchal and unfree core. To say that the "world we lost" was characterized by the subordination of the economy to the noneconomic values and norms of the community whereas market society has freed the economy from those moorings is to misrepresent the meaning of modernity, a meaning that has to be located in no small measure in a rejection of the household model in the economic sphere and its close analogues at the political level and in an attempt to fashion a new community and economy.[17]

Consider, as an application of this argument, that in market society, labor and land have been made into commodities, a proposition that is intended to show the degree to which the self-regulating market has taken hold of—and economized—society, even down to the level of such "fictitious" commodities as nature and human activity. A more natural construction of this process of "commodification" is that land and labor are removed from their status-determined, nonconsensual location in the social hierarchy. Land is freed from its attachment to patrimonial lineage, with (as Tocqueville observed) the resulting democratization of the agrarian economy. Labor is taken from its servile status (determined by gender and other ascriptive factors) and made a part of the self-ownership of the person, something to be freely disposed of. Viewed in that light, it is once again inaccurate to characterize the "great transformation" as one from a society in which noneconomic, human values ruled to a modern order in which the primary determinants are the all-embracing and purely economic laws of the juggernaut market. There was indeed a great transformation, but it might be better expressed as a move from a community and its economy (heavily encroached upon by hierarchy and status) to a society in which a certain equality and autonomy were accorded primacy.

A similarly contestable reading is at work in the way the embedded/disembedded distinction is used to weave the moral history of modernity as the battle between the self-regulating juggernaut market and a society trying to impose a certain humaneness upon it. A less tendentious reading is that voluntary (contractual) relations among persons, whether in economic or other matters, represent a central good in liberal market society and that therefore any interdiction of what is voluntarily agreed to among persons must be taken, in the first instance, as a prima facie evil. However, this is not to say that voluntary agreements are the only good to be secured, at whatever cost to other values society may wish to advance. It is to assert, however, that the interdiction of freely made agreements must be understood as a normative loss and weighed accordingly against the expected benefits. The many ways in which society now regulates contracts and markets represent not a reembedding of a purely economic sphere but the seeking of a balance among competing goods, one of which is the importance attached to the market as a forum composed of voluntary contracts that can, despite their importance, also on occasion be restricted so as allow for other goods to be realized. We could say then that what the moral economists call the reembedding of the economy is better explained as the workings of a society possessing a plurality of goods. In Michael Walzer's terms, the market is one sphere of justice, and when its operations "abut" on different spheres with other principles of justice, society may decide to limit its effects by regulating it in various ways (Walzer 1983). The *embedded/disembedded* conceptual framework obscures the character of market society by simultaneously understating the presence of recognizable and distinct economic behavior in archaic societies and insisting on too radical a detachment of the modern economy from its sustaining institutional and normative nexus.[18]

THE MORAL THEORY OF THE EMBEDDED ECONOMY

Here is roughly where we stand: the moral economists are committed to a theory of the radical exceptionalism of market society, a view that entails (so they maintain) that the mode of explanation of modernity must be similarly novel. Some are also given to the use of premodern (householding) models—the classical Greek especially (half of Polanyi's last major work, *The Livelihood of Man,* is devoted to ancient Greece)—as part of a critical apparatus employed against the world of the market. For reasons already suggested, I take the latter to be largely unsustainable in the late twentieth century; the former is a valuable invitation to further exploration of the issues surrounding the interpretation of economic forms. While acknowledging the difficulties to which the foundational *embedded/disembedded* contrast gives rise, what I wish to pursue further is the claim that far from being unserviceable, this approach suggests a powerful (and debatable) way of thinking normatively about the economy.

At the center of this part of the reconstruction of the moral economic argument is the idea of the embedded economy, and while, as I just said, that idea is not uniquely specified by the particular structure of the classical Greek *oikos*, the latter is nevertheless a good starting point. Because in linking *oikos* and *oikonomikē,* "household" and "economics," it intimates another understanding of the economy as located in the mesh of family (or community) relations and purposes.[19] We can approach this idea by unfolding it into the two parts of the meaning of Aristotle's idea of the economy *en tais koinōniais,* "in the framework of the community."[20] The first suggests that the provisioning of human beings is embedded or submerged in a nexus of relations—relations of justice, if you will. For example, on certain holidays, members of families give gifts to one another, and those exchanges are embedded in the framework of their community—their religious beliefs, their norms of deference and of reciprocal obligation between parents and children and so forth. By the same token, in a political community of equals who prize freedom above all, relations of distribution and of production will adapt to that overarching pattern. So, likewise, in a society that recognizes deep inequalities among its members, the economy will most probably take on yet another form. The economy, in short, is suffused by the norms of the community of which it is a part, and we would expect that where divergence occurs either economic relations will be refashioned so as to bring them into harmony with the community or the latter's values will, in a process of reflective equilibrium, undergo change. In this sense, and contrary to Polanyi-type arguments, all economies, including the near-to-pervasive-market economies, are moral economies, embedded in the (ethical) framework of their communities.

A second (and related) way of unpacking the concept of the economy *en tais koinōniais* is as a part of the view that all communities aim at some good, the perception of which informs the life of that association in its many and varies aspects (Aristotle, *Politics* 1252a1–5). We have just seen one manner of speaking about that claim, and the language of aiming or "being directed to" suggests another. From that perspective, the economy is understood as instrumental to the good, and that claim marks the beginning of a teleological inquiry about the good and about the place of the economy in relation to it. Here is a schematic version of this aspect of the classical theory as it proceeds from the starting point of the household. Wealth is an instrument or tool: Aristotle speaks of "wealth . . . and instruments generally" and again of property as "tools for the purpose of life," and Xenophon has Socrates discuss "the instrument wealth" (*organa chrēmata;* see Xenophon, *Oeconomicus* 2.13; Aristotle, *Nicomachean Ethics* 1987a28; idem, *Politics* 1253b31–32). Like all instruments, it has an end or purpose external to itself and in light of which its worth is judged. Wealth not governed by such ends does not constitute a good for human beings, because it has lost its telos-directed nature. Instead, it becomes a dead weight, a thing to be borne but that does not serve its proper end; or worse, it causes that peculiar corruption of the soul, *pleonexia,* the unlimited desire for things, a Midas-like passion that (as was the case with Midas himself) deprives those whom it afflicts of the ability to use properly the things they have.

So knowledge of the purpose that wealth ought to serve is essential to its character as an instrument in the service of the good—essential, in other words, to its being wealth at all. We do not recognize wealth as wealth (much less know how to organize and deploy it) before we have answered the query what end it serves—and not what derivative ends but, rather, what highest end for the person or the community. A plow is an instrument, and the slave is a "living tool" in the service of the *oikos* master, as also are his wife and overseers (Aristotle, *Nicomachean ethics* 1161b3–5; idem *Politics* 1253b24–54a8, 1255b30–40; Xenophon, *Oeconomicus* vii. 10–17, xii.12–20). They create material things for him (food, clothes, etc.) and by carrying the burden of direct production and supervision allow him time for higher activities. Yet none of these are wealth (at least not yet), for they await an answer to the question of how they are to be used, of the final good in relation to which they are but so many instruments. The person who is truly a good economist, who possesses *oikonomikē,* is the one who can answer that question. It surprises *us* perhaps, but not *them,* that Socrates, poor, pale, and shoeless, should lead Xenophon's discussion of economics; for he was, after all, "the chief of helpers in the quest of virtue" (Xenophon, *Memorabilia* 4.8.11).

Any human community—the household or the city—has thus to ask itself this primordial question concerning the location of the economy in its hierarchy of goods. Not to subordinate the economy to those rank-ordered goods is, in a basic sense, to render the community incoherent from the point of view of justice—to cause perverse distributions of things and to allow wealth itself to become the chief good. Classical Greek philosophers saw this subordination as directed to the master's leisure and praxis and those in turn to the possibility of philosophic or citizen activity. The classical city, for its part, intervened in the economy for the sake of securing food supplies but also to allow for the participation of poor citizens in democratic institutions.[21] In both cases, the embedded economy could be seen as the practical conclusion to an inquiry concerning the end or purpose of economic activity. Economics as the science of wealth—its acquisition and employment—must be guided by the question of wealth's purpose or render itself radically incomplete. It presupposes another science, a moral or political one, just as the object of its study, the specific institutional form of the economy, presupposes our common answer to the question, "To what end?"

The answers ventured by the ancients to this question, a rentier life for the *oikos* master and for the city, are now museum pieces, valuable primarily as a warning against romanticism. However, as I have just suggested, it is possible to tease out of those arguments the materials for an alternative framework within which to think about the economy. Because that framework makes economics—unavoidably and at the outset—an ethical inquiry, it is a worthwhile antidote to what Amartya Sen has called the "engineering" approach to economics, an approach that seems at times almost to revel in its nonethical character (Sen 1987, 2). For this framework points to two related paths, reflecting the dual types of inquiry that emerged from our analysis of *en tais koinōniais.* In the first place, it says that economic science is incomplete if it does not embed its studies in an overarching account of the location of the economy in the wider architecture of the community, of which the economy is simply one moment. Second, it tells us that this overarching account must be centrally informed by the question of the good to which the economy and its sustaining institutional nexus are, or ought to be, subordinate. In so doing, it directs us beyond the language of rights, self-ownership, and in general the liberal way of framing the normative study of the economy, for the question "To what end?" leads us to see the limits of a theory grounded in exclusionary rights and neutrality, a theory that (from the standpoint of the moral economy) neglects the most basic of queries as to what good is served by the economy. Taken in its entirety, this theory insists on the study of the community and its institutions grounded in the ethos that invests them with their life and form as central to an adequate and complete account of the economy. It insists, in other words, on the economy as something that we do, that involves us in certain patterns of relations with our fellow human beings and with nature, and for a purpose or end. The question "What good is thereby served?" is essential to understanding what we do when we do the things called economic—much more essential than grasping the most efficient way of doing them.

I said that the institutional and teleological senses of *en tais koinōniais* are related, and that relationship is evident in what we have just discussed. The end—the good aimed at—informs the institutional structure of the community, including the character and location of the economy within it. But the household-model teleological theory that we have been considering does not simply want to map out a (normative) form of explanation. Its interrogative path defined by the question "For what purpose?" and emphasizing the instrumentality of wealth in the light of that question was meant precisely to call into doubt the market and democracy as forms for the economic and political integration of the community and to challenge them as ways of life (see Booth 1994; Humphreys 1983, 12). We have already spoken of one dimension of this. The good *oikos* represented a regulated economy, hierarchically organized according to the respective worth (proportional equality) of its members, husband, wife, children, free servants, and slaves and subordinated to a sovereign good. The world of the emporium by contrast was anarchical, indifferent to the community and its order, and bound together by the pursuit of money alone. Democracy, like the market, is blind to the necessary closedness of community (Plato, *Republic* 563a) and to all but a narrow and partial facet of the good.

We can generalize this still further by observing again that that type of inquiry was intended to display the (related) limits of the market and democracy. This it does by driving the question of the good towards a telos, a ruling good (or goods).[22] Whether by the dramatic and ascending path of Socratic dialogue or in the more mundane Aristotelian variant, conceptions of the good are challenged and undermined until that terminus is attained. Their thrust is to show that not all goods (or lives) are equal and that a failure to perceive and to be governed by the highest possible good produces a radical deficiency. Neutrality and an egalitarian pluralism are the hallmarks of that deficiency, yet they are institutionally enshrined as the virtues of the market and democracy. This, in brief, is how the critique of them in its classical form is worked through. Democracy, like money, tends to make all things alike, for both are in their different ways egalitarian and thus oblivious of ranked distinction—the one to the worth of the person, the other to the use value of the thing for the attaining of the human telos. One result is that unequal things and persons are rendered (conventionally) equal by democratic citizenship and money respectively (Aristotle, *Nicomachean Ethics* 1131a25–33, 1133a20–23; idem, *Politics* 1280a8–17, 1281a–8, 1317b4–9; Plato, *Republic* 558c). Mirrored in the lives we lead as members of a democracy (and presumably of the emporium), our characters become, Aristotle writes, like masterless (*adespotois*) households in which one can do whatever one wants (*Nicomachean Ethics* 1161a8–9; *Politics* 1317b10; see also Plato, *Republic* 557b). One does as he wishes because the idea of a sovereign good (the master) is absent and in its absence all desires and pleasures, all life plans, are treated and honored equally. In other words, this equality and indifference as among persons and ways of life is possible where, in Plato's words, "the acropolis [of the soul is] empty" (*Republic* 560b, 561b–c). For Aristotle as for Plato, that is an empty soul and city, a failed life and an inferior community, and its root cause is that the question of the telos, of the good life for citizen and human being, has been neglected. The "cloak of many colors," the celebration of the multiplicity and diversity of lives and ends, is testimony to the incompleteness of these institutions (and souls), evidence that they have not only not come to the end of the interrogative path "For what purpose?" but that they have lost sight of it altogether.

Here then is the nub of the dilemma for contemporary moral economists who wish to travel down that path. There is an answer to the question, "For what end is the economy in this specific, market, form?" The answer might be expressed, "So that within this sphere I may do with my things, my labor, and so on as I wish, consistent with a recognition of the right of others to do likewise; in short, so that as far as is possible under conditions of interdependency, I may lead my life 'from the inside'." Such an answer, when completely developed, is very likely to issue in some variant of a theory of institutional neutrality among those life plans rooted in, for example, liberal conversational constraints that rule out attempts to legitimate power through claims to a knowledge of the good or to a personal superiority, in the idea of a modus vivendi in the face of moral heterogeneity or in the notion of an overlapping consensus (see, e.g., Ackerman 1980, 8–12; Larmore 1987, 129–30; Rawls 1993). If the moral economist allows the interrogative path to end there, what she has accepted is (arguably) a more reflective form of liberalism—guided by the question of the good rather than by a theory of rights alone, but a liberalism nonetheless.

If, however, she follows that road as far it was meant to be followed and as far as its internal logic suggests it ought to be, pluralism (the "cloak of many colors") and neutrality or an overlapping consensus in place of a comprehensive and sovereign good (the "empty acropolis") will not be satisfactory. What then presses onto the philosophical stage is (something very much like) a theory of the good as over against the shadow world of the market and democracy, the world structured so as to allow for a plurality of life-plans lived from the inside (see Rawls 1993, 134–135, 146n). In other words, because neutrality among competing life plans is (from this standpoint) at best a chimerical answer to the question of the good and of wealth and the economy as instruments in its service, the moral economist will be led beyond the threshold of liberal conversation. In brief, it is difficult to see a via media by which this part of the project can be pursued and not end up in a challenge to a social pluralism of ends and the norm of choosing one's own life plans. Yet even if that is so, there is still much to be learned from an engagement with the moral economic argument. For their writings draw us into a way of thinking about the economy that is radically at odds with both the economic approach to human behavior and institutions and with the rights-based theorizing that has long commanded the conceptual heights of normative economics. They invite us—political scientists and philosophers—to place the economy in a larger cosmos of human goods, to see it as permeated by the norms of the enveloping society and thus to look at it in a light different from the one that we are accustomed to. That, I would suggest, makes the encounter with the moral economists worthwhile, however far we may choose to follow them along the road they marked out.

Notes

1. For recent social-scientific discussions, see Bates 1983; Bates and Curry 1992; Hechter 1983; North 1977; Popkin 1979; Scott 1976; Taylor 1993.

2. The German terms *Gemeinschaft* and *Gesellschaft*, best known from Tönnies's (1979) study, resist translation into English, because our nearest equivalents, "community" and "society," are so close in meaning as to smudge just those distinctions that the German usage was meant to capture. The difficulty is not merely terminological, however, for the no-

tion, central to the moral economic argument, of a radical separation between two great epochs, their corresponding normative structures, and the theories needed to explain them is (as we shall see) questionable. Among contemporary communitarians, Alasdair MacIntyre recognizes the affinity of his own work with the moral economic school but does not develop it (1988, 211; see also MacIntyre 1984, 227–28).

3. Finley (1975) is critical of parts of Polanyi's project. Some of the classical historians cited in this essay (e.g., Vidal-Naquet and Will) develop arguments similar to those of the moral economists without, however, directly employing their writings; others such as Humphreys and Finley address them explicitly. Above all, many of them share with the moral economists a suspicion of the economic approach to the analysis of these societies. To the best of my knowledge, they have not engaged in the normative/critical dimensions of the moral economic project. Other classical historians are sharply critical of the moral economic school (Ste. Croix 1959–60; Veyne 1974, 1976). I shall draw on their writings to fill out points only thinly developed in the canonical "moral economy" literature, and these references should not therefore be taken as including them among the members of this school (if that designation is even appropriate for such a heterogeneous cluster of theories).

4. On the complexities of understanding investment in the classical Greek world, see Millet 1983.

5. For a canonical statement of the debate over the limits of scarcity, cf. Aristotle, *Politics* 1257b28 and 1323b7–10 and Menger 1976, 94–96, 98–99, 100–101, 289–90.

6. To see the quite different interpretations given to the phenomenon of premodern trade, compare Mancur Olson's commentary on Herodotus' *The History* 4.196 describing trade between Carthage and native peoples (1992, 60–61) and Polanyi's gloss on the same passage (1968, 243; cf. 238–39, 241, 258). In general Polanyi emphasizes the administered character of premodern trade, the absence of an economic rationality, and the distinction between markets and trade. Olson's reading underscores the view that the "gains from trade are often huge" even in "the most difficult and primitive circumstances," rejects value-based explanations of the presence of markets, and suggests that the absence of a vital institutional nexus (e.g., courts and a legal system) prevents further such gains.

7. For a critique of Becker on the family, see Sen 1984, 372–73, 375; see also Sen 1990.

8. On subsistence, see Evans-Pritchard 1940, 84–85; Scott 1976; Thompson 1971. On labor, see Polanyi 1957c, 82–83; Thompson 1967. On the environment, see Polanyi 1957c, 73, 130; Wolf 1971, 277, 279–80.

9. Tönnies was wary, however, of what he took to be a romantic misappropriation of his book, one emphasizing a "social pessimism" about modernity.

10. On this and related issues, see Coase 1988, 3; North 1977, 708–11, 713; idem 1981, 20–33, 42–43, 106, 181; Posner 1983, 146–173.

11. I develop a critique of this aspect of the moral economic argument in Booth 1993a, 76–92.

12. On kinship groups as "insurance companies" and on open fields in peasant agriculture, see, e.g., Bates 1983, 7–20; Becker 1981, 238; Popkin 1979; Posner 1983, 146–173.

13. On Scott's mode of explanation, see Booth 1993b; Taylor 1993, 109–10.

14. For nonmoral economic readings of the ancient economy, see Lowry 1979; Gunderson 1982; Veyne 1976, esp. 69–74 (critique of Polanyi).

15. For a parallel critique of communitarian political philosophy, see Homes 1993, 190–97.

16. See Desai 1986, 8, On Marx though his comments are equally applicable to the moral economy argument. The difficulties encountered by the emerging market economies of the former Soviet bloc illustrate just how important this sustaining institutional framework is; see Clague and Rausser 1992.

17. A comparison of the *Gemeinschaft* to *Gesellschaft* estimation of this watershed with Maine's (1864, xl, 163) positive valuation of the shift from status to contract is instructive in this regard.

18. For an explanation of the firm as an embedded institution, see Granovetter 1985, 487–504. This article is a powerful sociological critique of both the economic and embedded approaches.

19. On the philological background to this, see Singer 1958.

20. I follow Will's suggestion and translate Aristotle's *en . . . tais koinōniais* (*Nicomachean Ethics* 1132b32) by "framework of the community" (1954a, 215). Aristotle is here discussing reciprocity, but the idea clearly informs the entire economic discussion of book 5, as well as the analysis of the household in the *Politics*, book 1.

21. On food policy, see Garnsey 1988; on civic salaries, see Will 1975.

22. For analyses suggesting a range of goods in Aristotle, rather than a single "grand end," see Broadie 1991, 198–202; Nussbaum 1986, 352–53, 498, n. 20; Salkever 1990; Saxonhouse 1992, 185–232. Relatedly, see Yack 1985, 94, 99, 109; idem 1993, 25–87 (community, esp. 43–50), 106 (democracy), 170–71 (political dialogue and the common good). Rawls 1993, 134–35 (and even more so MacIntyre 1984, 146–64) stress the more restrictive perfectionist reading, though needless to say with very different conclusions.

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THE PIETY OF THOUGHT IN PLATO'S *REPUBLIC*, BOOK 1

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In the opening sentence of the Republic, *Socrates recounts his intention to combine the operations of piety and theoretical speculation. Nevertheless, many readers regard Cephalus' subsequent abandonment of rational inquiry to perform certain sacred rites as a definitive indication of Plato's opinion that piety and philosophy are fundamentally incompatible. I find this interpretation untenable inasmuch as it depends upon the misidentification of Cephalus as the dialogue's representative of piety. I suggest that the true nature and philosophical significance of piety are indicated instead in Socrates' conversation with Cephalus' son, Polemarchus. As this conversation unfolds, Polemarchus' pious inclinations culminate in a perception of the dearness of the unknown good. Inspired by this piety, Socrates and Polemarchus defend the conventional paragon Simonides and, at the same time, launch a truly philosophical inquiry into justice.*

Plato's *Republic* is reputed to be one among a handful of the greatest works ever written on the theme of social justice and personal happiness. Yet if we should wish to verify this judgment, and to derive the benefit of its insight for ourselves, we would confront an extraordinary challenge in the complexity and sheer size of the work. This challenge might seem to outrun every endeavor, for the better acquainted one becomes with the *Republic*, the more time-consuming becomes the consideration of any given section of it. The pace of successive readings tends to slow. Eventually one opens the text only to ponder at length the significance of a single phrase or dramatic event. Such a meditation can be exquisitely pleasant. But what is there to prevent so closely focused a study from losing its bearings? How might the student keep from losing sight of the forest for the trees?

It would be incongruous, to say the least, were a professional academic to deny the relevance of this dilemma to the study of Plato's *Republic*. For it is the achievement of *synopsis* (the capacity to see simultaneously both the forest and the trees) that the *Republic* submits as evidence of a dialectical nature and thus as testimony of one's being qualified to undertake advanced studies in the first place (537b8–c7).[1] So it is incumbent upon every reader, when approaching this dialogue, to consider how the forester's dilemma might be resolved. Now, Socrates himself offers a key to the resolution of this dilemma in his remarks on the art of writing (*Phaedrus* 264c2-266c1). In a properly constructed composition, he maintains, one finds something like the integrity of a living body. Where such integrity obtains, the natural articulations or joints of a composition mark off segments that are intelligible precisely as parts of a whole. We may say, then, that the very divisions that call for detailed study also summon, at the same time, a recollection of the whole. By concentrating attention on such divisions, the student is able to think and to talk about the most minute textual details in a wholly nonreductive manner. Plato's Eleatic Stranger, expert in the art of dialectic, invites us to call such divisions *eidetic* or *ponderable* parts in order to distinguish them from the mere pieces into which any whole may be thoughtlessly—or even methodically—broken (*Statesman* 262c8–263b11).

In such passages Plato encourages the practice of dialectical analysis upon his own writings. But on what basis may we undertake the dialectical task of dividing Plato's *Republic* into ponderable parts? Clearly, some grasp of the dialogue as a whole is required before one can reasonably venture upon such a division. But whence comes this division-initiating understanding of the whole? Surely, it cannot originate in a concentrated study of ponderable parts, for this begs the question of which parts are correctly identified as ponderable. Neither can it derive from blind trust in the judgment of more experienced students, for this begs the question of the basis of the more experienced student's understanding. The only escape from this impasse lies in the possibility that the understanding needed to guide the division of the dialogue into ponderable parts is somehow accessible even to the novice upon an initial reading of the entire work.[2] The contours of the very surface of the work might then be said to reveal its natural articulations. Leo Strauss put it well when he declared in the case of such a work that "the problem inherent in the surface of things, and only in the surface of things, is the heart of things" (1958, 13).

Plato employs his luminous poetic talent accordingly, to epitomize the profoundest themes of his philosophical compositions. One of his customary devices in this undertaking is the ethological mime, the representation of an action in which all pretense is stripped away and an interlocutor reveals himself both in character and in thought (see Klein 1965, 18). The first example of such a *mimēsis* in the *Republic* reaches a climax early in book 1, with the departure of Cephalus. The importance of Cephalus' departure is further highlighted by its conspicuous reversal of the dialogue's dramatic momentum. In the beginning, Socrates and Glaucon are together. These two are soon joined by Polemarchus, Adeimantus, Niceratus, and some others. This ensemble then makes its way to Polemarchus' house, where await Lysias, Euthydemus, Thrasymachus, Charmantides, Cleitophon, and Cephalus, the father of Polemarchus. A conversation is presently launched concerning the nature of justice. Although this conversation continues into the early hours of the morning, not a single member of the congregation is said to depart, except for Cephalus. Cephalus abandons the inquiry in its infancy. The old man departs because, as he says, "the time is come when I must tend to the sacred things" (331d7).

Cephalus' departure surely stands as one of the most striking dramatic events in the dialogue. As such, it invites comparison with the violent eruption of Thrasymachus, who hurls himself at Socrates and Polemarchus "like a wild beast" (336b5–6). But the antagonism represented by Thrasymachus' outburst is composed in the course of the conversation. Socrates later states that he and Thrasymachus have become friends, though they were not even enemies before (498c6–d1). Cephalus' departure, on the other hand, is final. He never returns. So if even Thrasymachus (the sometime champion of might over right) can be reconciled to Socrates, it would seem that Cephalus' complete break with Socrates and the community of inquiry must portend an issue of considerable importance. Following the clue of Cephalus' parting words, we may infer that this issue somehow concerns the divergence of philosophical inquiry and attendance to "the sacred things." Cephalus' departure somehow manifests Plato's understanding of the tension that exists between the interrogatory motion characteristic of the philosophical life and the magisterial stability characteristic of revelation and fundamental law. Strauss characterizes this tension—the philosopher's "theologicopolitical predicament"—as nothing less than "the secret of the vitality of Western civilization" (1989, 270).[3] Of course, one need not agree with Strauss's specific interpretation of the character of this tension to agree with his estimate of its vital importance. But any attempt to propose a different interpretation must account for precisely such testimony as is presented by Plato's rendition of the departure of Cephalus. We may begin to offer such an account by noting that Cephalus' schism constitutes the seismic epicenter of a passage bounded by the opening of the dialogue on one end and by the closing of Socrates' conversation with Polemarchus on the other.[4] As we shall see, this passage must be considered in its entirety if we are to arrive at an accurate assessment of Plato's conception of the relationship between piety and philosophy.

THE DIVERGENCE OF PRAYERFUL REVERENCE AND SPECULATIVE SCRUTINY

Let us begin then at the beginning, for Cephalus' parting words are not the first indication in the *Republic* of a concern for the relationship of piety and inquiry. In the dialogue's opening sentence, Socrates reports his own intention of combining these divergent operations. To appreciate the full significance of this remark, however, we must keep in mind that the *Republic* is, in form, a narrated dialogue—and a particular kind of narrated dialogue at that. Plato assigns the role of narrator in the *Republic* to Socrates. In this role, Socrates reports not only what he and others said and did on the preceding day but also what he thought then and what he thinks now, with the benefit of hindsight, of those words and deeds. So Plato puts Socrates forward as a uniquely authoritative commentator on the conversation reported in the *Republic*. Nevertheless, even in his narration Socrates leaves many things unexplained. For example, the occasion of his recapitulation of the previous day's doings is not itself described. Nor is there any indication of the existence (much less the identity) of any auditor or auditors. Socrates, to all appearances, is talking to himself. Of course, we readers are the intended audience—ultimately. But within the frame of the dialogue, Socrates addresses his rehearsal to no one but himself. The reader evidently is witness to an exercise in personal recollection, a soliloquy.[5] Now a soliloquy is a speech spoken with a level of candor not to be expected in the public discourse of a prudent man. So Socrates' narrative remarks will be more directly indicative of his own views than much of what he reports having said in direct discourse with his interlocutors, whose dispositions might on occasion be expected to skew, to some extent, his choice of words.[6] We are well advised, then, to consider carefully Socrates' opening statement of purpose: "Down I went yesterday into Piraeus with Glaucon, Ariston's son, both in order to pray to the goddess and at the same time because I wanted to see in what manner they would conduct the festival, inasmuch as this was the first time they were holding it" (327a1–3).

In this narrative opening, Socrates cites a thought-provoking linkage between the two reasons that draw him toward Piraeus. He descends "*both* in order to pray . . . *and at the same time* because [he] wanted to see" (*te . . . kai hama*).[7] It is important that we understand the significance of this linkage correctly. For some might suppose that Socrates is interested in nothing more than "killing two birds with one stone," in the sense of indulging his personal intellectual curiosity (*theasasthai* 327a3, *theōrēsantes* 327b1) while merely appearing to satisfy certain social expectations concerning his acknowledgment of the goddess. But Socrates does not say that he intended to be seen praying; what he says is that he intended to pray (*proseuxomenos* 327a2). Nevertheless, many readers will be inclined to maintain that any show of devotion on Socrates' part must be construed as nothing more than the window dressing required if the philosopher is to escape persecution by an intolerant, closed society.[8] On this interpretation, Socrates bows not to the goddess but merely to his own theologicopolitical predicament.

This interpretation deserves to be taken seriously, for the *Republic* certainly presents convincing evidence that Socrates rejects many points of theology popularized by Homer and Hesiod. There is also the matter of his claim to possess a personal *daimonion*, which undoubtedly overshot the boundaries of Greek religious convention (albeit in the manner of evidence of Socrates' acknowledgment of the existence and the providence of the divine). But these considerations by no means preclude the possibility that Socrates finds himself obliged to the graciousness of certain other divinities, not all of which were utterly unrecognized by the customary religion (see, e.g., *Phaedrus* 279b8–c3, *Republic* 451a4–5). Indeed, other factors besides the candor of his narrative remarks corroborate the sincerity of Socrates' avowed intention to pray to "the goddess." For the goddess in question is Bendis, as seems clear from the sequel (354a10–11).[9] Bendis is the Thracian persona of Hekate (a.k.a. Artemis), leader of souls in the underworld and goddess of crossroads (Pauly-Wissowa 1937, 3:269; Brann 1967, 111, n.4; Burkert 1985, 171; Morgan 1990, 21). Such a goddess might well be reckoned a patroness of Socratic political philosophy, for in one of the most celebrated metaphors in all the works of Plato, Socrates likens the political realm to an underground abode, a cave, which he in turn compares to Hades (514a–15a5, 516c8–e7). Socrates "goes down" into Piraeus, just as Heracles went down into Hades to perform certain labors (Brann 1967, 3–6) and as the philosopher king is later said to go down into politics (540b5–6; see Dobbs 1985, 812–21). Socrates makes this descent with Glaucon, a young man who stands at a *moral crossroads*. Glaucon faces a choice between justice and injustice, a choice that will determine the course of his life and the salvation of his soul (347e2–7, 352d4–6, 621b8–c2; see Dobbs 1994).

Moreover, if Bendis does represent some aspect of the true divinity (the one necessary and eternal being, recognized by philosophers of every era), Socrates' prayers to the goddess would naturally express and comport with his own acknowledgment of mankind's finitude and contingency. An appreciation of the limits of the human condition is crucial to the understanding of Socrates. Socratic wisdom is not divine wisdom. Yet Socrates also holds that human beings are capable of partaking in the heavenly spark of divine intelligence (e.g., 590c8–d6). For Socrates, then, the human condition is twofold in nature. Consequently, a pair of prerequisites must be satisfied for a human being truly to know himself: each of us must acknowledge the gulf separating himself from the divine and, at the same time, honor his own capacity for divine intelligence by developing it to its highest peak of excellence. Such self-knowledge would expose the mortification of ascetics and the pride of intellectuals as inadequate and merely piecemeal responses to the human condition. For what the human condition calls for is *both* reverential *and* intellectual endeavor, not merely severally or alternately but *at the same time*. So Socrates' twofold intention—both to pray and at the same time to see—is indicative of the character of an ordinate and (I hazard to say) just response to the essential human calling. Of course, it remains to be seen whether and how far such a response is practicable.

What, then, are the prospects for the satisfactory realization of Socrates' intention? It seems evident that the operations of reason and reverence may collide. Piety is as demanding in its call for commitment as critical judgment is in its requirement for detachment. Each would appear ultimately to exclude the other. If we take into consideration the conversation occasioned by Socrates' descent into Piraeus, we find that Cephalus' abandonment of Socrates' inquiry into justice—at the very moment that inquiry appears to intrude upon his service to the divine—dramatizes this conflict and suggests the *possibility* that piety and rational inquiry are fundamentally incompatible. This possibility shades increasingly into a certainty as one looks upon Cephalus as the embodiment of genuine piety. If Cephalus does stand for piety, then one might well conclude that it is simply impossible for the same person to do justice to both aspects of the human condition (Strauss 1989, 269–70).

But it is by no means clear that this is Socrates' view, or Plato's. Certainly, one cannot leave unexamined the possibility that it is Socrates, rather than Cephalus, who practices genuine piety. In fact, Socrates' disclosure of intent in the narrative opening of the *Republic* corroborates the public testimony he offers later at his trial on the charge of impiety. In this testimony, Socrates answers the Athenians' notorious indictment by portraying his customary cross-examination of fellow citizens as a service to god. He explains, however, that this pious service involves, at the same time, a scrutinization (*elexōn*) of the divine oracle itself (*Apology* 21b1–23c1). In other words, Socrates interprets his divinely sanctioned vocation as one that requires a combination of piety and rational scrutiny.[10] Plato's illumination in the *Republic* of the quality by virtue of which Socrates can achieve this combination will succeed, however, only if certain pretenders to the status of reverence and reasonableness are first unmasked. This, to state the matter concisely, is the guiding purpose informing his composition of Socrates' conversation with Cephalus.

THE EXPOSURE OF RATIONALISM IN CEPHALUS' FALSE PIETY

As Socrates and Glaucon head homeward from the sacred processions, they are intercepted by Polemarchus' servant. Polemarchus and Adeimantus arrive presently and prevail upon Glaucon and Socrates to go to Polemarchus' house. There Socrates meets with Polemarchus' father, Cephalus, who has just come from performing some sacred rite himself. Cephalus greets Socrates warmly. Pleading his own inability to make the journey up to the city, Cephalus urges Socrates to come to the Piraeus more frequently.

Socrates says he delights in conversing with his elders, for it permits him to inquire about the path that he, too, may have to travel one day. So he asks Cephalus, "Is it a hard time of life or what?" Cephalus' answer is heartening. This old man discounts the many complaints one ordinarily hears from old men. Their nostalgia for the faculties and pleasures of youth is especially mistaken, according to Cephalus. As against such complaints, Cephalus notes that even the tragedian Sophocles, high priest of the Dionysian celebration of eros, regards the dwindling of sexual appetite as a liberation from a raging tyrant. To mourn such a liberation is absurd. The complaints and the troubles of the aged must be blamed, then, not on old age but on bad ways (329d2–6). After all, if it were old age itself that caused such trouble, Cephalus reasons, he would suffer from such difficulties as much as the next man.

Socrates marvels at Cephalus' saying this and desires to hear more. He notes provocatively that many would be likely to dismiss Cephalus' testimony altogether; they would credit his brave words to the possession of wealth. Cephalus concedes that wealth is important, though not so important as the multitude may suppose. By way of explanation, he recounts a saying of the illustrious Themistocles. It seems that a certain "Seriphian detractor"—a man embittered by his own obscurity, which he blames on the political insignificance of his native city—once insisted that it was only thanks to Themistocles' being an Athenian that he had achieved glory. In reply, Themistocles acknowledged that if he too had been a Seriphian, he would never have become renowned; but neither would this detractor ever have become renowned, even if he had been an Athenian! Cephalus thus suggests an analogy between his own conception of great wealth and Themistocles' conception of great politics. Cephalus no more regards wealth as an assurance that one will possess the greatest good than Themistocles so regarded citizenship in a great polity. Socrates surmises that Cephalus has inherited his wealth, because he does not seem in what he is saying to be overly fond of money (330b8–c1). Those who earn their own wealth, in contrast to those who are given it, resemble parents or poets. The latter cherish their children or poems not only insofar as these are good or serviceable (*kata tēn chreian*) but also because they are their own (330c3–6). Socrates thus remarks on the absence in Cephalus of a vehement (*sphodra*) love of his own. It seems then that the Themistocles analogy may run deeper than Cephalus had intended, for, as we know, Themistocles betrayed his own country, showing himself in the end to be indifferent as between the equally serviceable political communities of Athens and Persia. Cephalus, we may say, is similarly indifferent as to the source of his wealth, provided only that it proves to be of service to him. The precise character of Cephalus' understanding of the serviceability of wealth remains, then, to be considered. So far, Cephalus has suggested that wealth is some sort of prerequisite to the enjoyment of happiness.

One wonders in what sense wealth is prerequisite to happiness. In Cephalus' case, the relationship between wealth and citizenship exceeds the analogical and approaches the literal, for as a resident alien, or metic, Cephalus is not eligible to participate in the politics of Athens. For all practical purposes, Cephalus' wealth *is* his country.[11] But to Socrates, who proclaims that he never left Athens except to fight in her defense and who suggestively adverts to the riches of his own *ten-thousand-fold* poverty, Cephalus' likening of wealth to citizenship must appear questionable. So Socrates invites Cephalus to name what it is that he takes to be the "greatest good he has enjoyed from his abundant wealth" (330d2–3). Cephalus declares that thanks to his wealth he has not been forced "to cheat or to deceive anyone, or to depart for that next place in fear, on account of owing some sacrifices to god or money to a man" (331b1–4). Of course, Cephalus stipulates that wealth does not provide this benefit for just any man but only for one who is decent and orderly (*tōi epieikei kai kosmiōi*). Couched in this qualification, Cephalus' evaluation of wealth might seem innocuous enough. One is almost tempted to equate it with Aristotle's appraisal of wealth as the "necessary equipment" of a happy life (*Nicomachean Ethics* 1101a14–16). But we must tread carefully here, for Cephalus values great wealth not as an instrument employed in the exercise of an already confirmed disposition to virtue but rather as a necessary means to confirm such a disposition in the first place. Its possession provides insulation from circumstances that Cephalus assumes will drive any man to crime. But for Aristotle it is precisely character that keeps a decent man "in character." As it happens the decency stipulated by Cephalus is not a matter of character (*ēthos*) in the strict sense at all. Instead, it is in his own words merely a *tropos*, that is, an inclination or tendency (see 329d3–4). Wealth, according to Cephalus, is the functional equivalent of character.

Once this has come to light, Socrates admiration for Cephalus gives way to thinly veiled irony.[12] Nevertheless, Cephalus' words and deeds continue to demonstrate that he is a man very much concerned (one might even say preoccupied) with ultimate or eschatological matters. This does not, of course, establish his piety or righteousness. On the contrary, the aged Cephalus experiences what may be described as a servile, rather than a reverential, fear of the gods. It is only due to anxiety at the prospect of retribution in the afterlife for his crimes in his present life that the anerotic Cephalus even takes notice of the gods: "Know well, Socrates, that when one is gripped by the thought that his end is near, he is possessed by fear and concern for things that never occupied him before. The stories, which he used to ridicule, about what goes on in Hades—that the man committing injustice here must pay the penalty there—now torment his soul for fear they might be true" (330d4–e2). (A truly erotic soul, however, would not require the prick of such anxiety to seek the divine [see, e.g., *Phaedrus* 248d2–249e4].) Of course, Cephalus is uncertain whether his newfound interest in sacred matters is a function of some special perspective vouchsafed the aged by virtue of their proximity to death or is merely a function of the infirmities of old age (330d7–e5). But he does acknowledge that he finds security in assuming the worst as regards the eschatological facts of life. Cephalus compares what he has to lose if he mistakenly disregards the warnings of traditional theology with the value of the money required to carry out the customary, propitiatory oblations. He decides to sacrifice his superfluous wealth, which he takes to be all except enough to leave his heirs a little more than what his own spendthrift father left him after wasting a good portion of the family inheritance (330b6–7). In other words, Cephalus makes his gamble on the same terms as the famous wager, which, we may be assured, Pascal contrived to demonstrate the untenability of the rationalist criticism of piety, rather than to induce genuine faith (Pascal 1986, §418 and §110). Hence, Cephalus' turn to religious ceremony is hardly pious in itself; it is merely indicative of a certain talent for calculation that is pressed into the service of anxiety concerning the afterlife. It follows that Cephalus is better understood as a representative of rationalism—which holds that rational calculation suffices for the resolution of all questions of human affairs—than as the spokesman for piety.[13]

Of course, there can be no question but that Cephalus embodies the mercenary tendency that the vulgarization of Orphic writings and religious practice had taken in his time (see, e.g., 363c3–365a3, 365d7–e6). In this sense, one may speak with some propriety of Cephalus as representative of a *decadent* piety. But traditional piety in its decadence is by no means to be confused with traditional piety in its prime (see, e.g., Morgan 1990, 108–14). Hence, I suggest that Cephalus' divine service bears roughly the same relation to traditional Greek piety that Macy's Christmas parade bears to the pilgrimage of the Three Wise Men. To be sure, Cephalus' departure from the conversation indicates the incompatibility of a decadent piety with Socratic philosophy, but it in no way settles the question whether or not Socratic philosophy is at odds with uncorrupted piety. Therefore, if, as Bloom says, "Socrates must induce Cephalus to leave the scene because Cephalus is beyond reason" (1991, 312), we must add that it is not due to piety that Cephalus is beyond reason. We may grant that Cephalus leaves because he is beyond reason, but he is beyond reason only because there is something unreasonable in being a rationalist.

In the course of answering Socrates' question concerning the greatest good that derives from his possession of great wealth, Cephalus had occasion to imply that it is unjust to cheat or lie to any man or to fail to render to gods or men the sacrifices or things they are owed (331b2–3). Picking up on this suggestion, Socrates now asks whether we should pronounce "the opposite of these things, namely the truth and the rendering back of what one has received from another, to be justice or righteousness itself; or should we say that it is just in some cases and unjust in others to do these very same things? (331c1–5). Cephalus does not respond. He has not taken the trouble to formulate definitively his understanding of righteousness, whether in relation to other human beings or to the gods. What he has is a rule of thumb. But Socrates pursues the inquiry. He proposes a thought experiment that may help Cephalus to sort out the essential from the merely accidental characteristics of acts of justice. Socrates supposes that everyone (*pas* 331c5) would agree that it would not be just or right to return a borrowed weapon to its owner, if the owner were to demand it in a fit of madness. Cephalus acknowledges, in response, that Socrates speaks correctly (331d1). Hence, there can be no question but that Cephalus correctly judges the force of Socrates' counterexample. The most we can say is that Cephalus fails to follow up this correct judgment with an attempt to formulate more adequately his understanding of justice itself.

Cephalus, in other words, is complacent in the face of the manifest inadequacy of the definition that Socrates has cobbled out of his rule of thumb. But this is neither irrational nor contrary to expectation. The fact of the matter is that Cephalus correctly judges the test case. (Of course, this is no great claim, since Socrates is willing to assume that it is true of everyone. We shall consider Polemarchus' taking exception with this momentarily.) In other words, Cephalus rightly perceives that the situation described by Socrates does not properly fall under the jurisdiction of his rule of thumb. But a genuine refutation would require that Socrates devise a case in which Cephalus *errs* either in applying or in failing to apply his rule of thumb. Socrates does not do this. Logically (but only logically), then, Socrates and Cephalus are quits. Polemarchus steps in at this point, oddly enough, to defend his father's rule of thumb *as if it were a definition* (*horos* 331d2–5). Polemarchus' initiative provides Cephalus with an opening to depart. The old man speaks of handing the argument down to the assembled company (*hymin*, 331d6). But Polemarchus persists in taking matters into his own hands: "Then am I not the heir of what is yours?" (*egō*, cf. *toutoisin* 330b6). Cephalus laughs at his son's enthusiasm, uttering something like, "It's all yours, such as it is" (*Pany ge* 331d9), as he departs to attend to the sacrifices.

The palpable contrast between Cephalus' appreciation of the force of Socrates' counterexample and his son's zeal to defend the indefensible suggests that the shortcoming of Cephalus, anyway, is hardly one of rationality. Moreover, as we have already noted, Socrates does not refute Cephalus. What Socrates does accomplish in their exchange is to reveal with perfect clarity the nature of Cephalus' temperament. This revelation is a critical step in the disclosure of Plato's understanding of the relationship of piety and philosophy, for Cephalus represents a common vulgarization and misunderstanding of piety. As long as Cephalus stands as a representative of piety, the relationship of piety and philosophy will be misconstrued. Plato corrects this misunderstanding through his ethological mime, in which he strips away Cephalus' pretense to piety and reveals that Cephalus' turn to religious ceremony is thoroughly mercenary, not reverent. Hence, we may conclude that Cephalus' departure from the dialogue in no way dramatizes the incompatibility of reason and piety. If anything, his departure is indicative of the divergence of rationalism from the virtue of reasonableness. Rationalism, thus understood, is an obstruction to the mutual opening of reason and reverence. With the departure of rationalism, this obstacle is removed.

THE PLACE OF REVERENCE IN POLEMARCHUS' TURN TO PHILOSOPHY

Cephalus, as already noted, does not attempt to universalize his rule of thumb. But Polemarchus rushes in where his father is too shrewd to tread. Polemarchus holds that his father's formulation does provide an adequate definition of justice, "at least if we are to be persuaded by [or to obey] Simonides" (331d5). Polemarchus thus acknowledges a requirement (*chrē*) to defer to Simonides. Socrates by no means disputes this requirement; he grants that Simonides is a man of wisdom *and* godliness (331e6). So he invites Polemarchus to identify "what it is you suppose that Simonides said correctly in speaking about justice" (331e1–2). Polemarchus thinks Simonides speaks not just correctly but nobly (*kalōs*) when he states that justice is rendering to each what is owed (331e3–4). We must note, however, that what Polemarchus espouses here is an unwarrantedly literal-minded interpretation of the poet. By the term "what is owed," Polemarchus evidently assumes that Simonides means exactly what another has lent him. Hence, Polemarchus deems superfluous the question of the mental capacity of the man in Socrates' thought experiment, the relevance of which even his father had appreciated. On this basis, he holds that his father's rule of thumb may be enforced over the entire range of just actions. Polemarchus maintains that it is just and right, regardless of circumstances, to return a borrowed weapon to its original owner, because such a weapon is plainly owed to its owner and because justice consists in returning what is owed.

But before we go too far in disparaging Polemarchus' literal-minded veneration of Simonides, we must pause to ponder the significance of the manner of Socrates' questioning. Socrates asks no more of the young man than to declare what it is that he supposes Simonides says correctly. In other words, Socrates does not impose upon his interlocutor the Cartesian burden of proving beyond all possible doubt the truth of what Simonides says. Socrates thus avoids calling the wisdom of Simonides into question (see Adam 1902, 1:13; Bloom 1991, 316). But the significance of this insulation of Simonides from the rigors of Socratic elenchus is commonly mistaken. Many readers see in it no more than the tact (and perhaps the tactic) of a philosopher who prudently camouflages his own awareness of the shortcomings of a conventional paragon, which an intellectually inferior interlocutor is supposedly incapable of appreciating. Such a view, however, assumes more than an inequality between the interlocutors; it posits a discontinuity between what Socrates knows in this matter and what lies within the capacity of Polemarchus to understand. It presumes that Polemarchus is unable to learn what Socrates can already discern. (The reader may wish to consider the extent to which this view implies a certain dogmatization of the essentially negative character of Socratic wisdom.) As an alternative to this view, I would simply suggest that Socrates' respectful treatment of Simonides is informed by his appreciation of the possibility that Simonides might well be right and by his collateral desire to help Polemarchus to acquire a similarly thoughtful appreciation. In other words, I am suggesting that Socrates reads Simonides in the way we have undertaken to read Plato. Socrates' mode of questioning Polemarchus encourages the lad's confidence in the poet's wisdom and, at the same time, calls upon him to exercise his critical abilities in identifying the formulation that best expresses this wisdom. We should note that Socrates endorses such a combination of conviction and discrimination throughout this dialogue on justice. He holds that this combination will ensure that inquirers will be, as he puts it, "both judges and advocates at the same time" (348b3–4). Socrates' locution, "*both* judges *and* advocates *at the same time*," recapitulates the linkage of the divergent operations of piety and reason, advocacy and criticism, which was suggested in the dialogue's opening sentence. But it is Socrates' own conduct in the inquiry that most strikingly dramatizes the correlation of these divergent operations, for he steadfastly advocates the cause of justice as *sacred* (368b7–c2) even as he conducts a searching investigation of its nature and goodness.[14] It is no surprise, then, that Socrates proceeds as he does in his questioning of Polemarchus. He summons Polemarchus to be both an advocate and a discriminating judge of Simonides. By responding as Socrates directs, Polemarchus becomes a full participant in Socratic inquiry.

So rather than ridiculing Polemarchus' literal-minded veneration of Simonides, Socrates attempts to encourage its development. He indicates that the phrase "to each what is owed" cannot be meant literally, for no one would say that a borrowed item is to be returned *to anyone whatsoever*, should its owner make such a demand when out of his mind (331e3–4). Socrates' nudge opens Polemarchus' eyes. It must mean "something else, by Zeus," Polemarchus now realizes, "for [Simonides] thinks that it is owed *to friends* to accomplish good and nothing bad" (332a9–10). Polemarchus reasons that if something bad results from returning a weapon to a friend who is out of his mind, it cannot be right to have returned what one has borrowed. Polemarchus' devotion to friends works as a lever to dislodge him from the grip of a narrowly literal and unwarrantedly dogmatic interpretation of Simonides. Interestingly, his recollection of friendly solidarity has made it possible for him to exercise critical thought. Polemarchus now judges that it is the poet's statement on friendship—that it is owed to friends to accomplish good and nothing bad—that is his most important dictum. Consequently, the lad abandons his literal interpretation of "what is owed" without in the least abandoning Simonides. Polemarchus merely exercises the prerogative of interpreting Simonides out of Simonides, just as the classical formula for sound exegesis recommends. If attention to the whole of an authoritative body of writings demonstrates more respect for an author than does mere proof-texting, we may say that Socrates has already succeeded in bringing about an improvement in the quality of reverence Polemarchus is prepared to show Simonides.

As we have noted, this refinement in Polemarchus' reverence follows upon his own recollection of friendly solidarity. A hearty concern for that which is dear (*to philon*) is characteristic of the psychic capacity that Socrates terms spiritedness (*thymos*).[15] Polemarchus, whose name evokes the fiery spirit of a warlord, momentarily overlooked the special claims of friendship when he formulated his argument for the justice of returning deposits always and everywhere. He quite naturally swears an oath as he recovers himself and corrects this oversight. Polemarchus' vehemence combines anger for the past affront to friendship and zeal for its present correction. Socrates invites Polemarchus to complete the formulation of his understanding of Simonides' saying concerning justice, now that he has the special claims of friendship clearly in mind. "What about enemies," Socrates asks, "are we to render whatever it is that happens to be owed to them?" (332b5) "By all means," Polemarchus replies, "we are to render to them exactly what is owed, which I take to be the very thing that suits [*prosēkei*] an enemy from an enemy, namely something bad [*kakon*]" (332b6–8). So, according to Polemarchus' revised interpretation: (1) Simonides' phrase "what is owed" is best understood not literally but rather analogically, as the *suitable* or *the fitting* and (2) the suitable or fitting in the case of enemies is something *bad*. Polemarchus thus proposes that what Simonides means by justice is rendering something good to friends and something bad to enemies.

It may be inferred from Polemarchus' oath ("by Zeus!") that he has his heart—that is, his *thymos*—set on this definition of justice. It appears that Polemarchus' most heartfelt desire is to uphold and to do right by the distinctive status of friends. Socrates must nevertheless subject this understanding of justice to a searching examination if there is to be any further refinement of Polemarchus' piety. Such an examination calls for great delicacy, to say the least. Socrates conducts this examination in three steps, the outlines of which may be briefly indicated in anticipation of more detailed discussion below. First, because *thymos* is invincible (375a11–b1, 440d1–3), one cannot oppose its present orientation directly. Instead, Socrates seeks to disengage the spiritedness with which Polemarchus embraces the notion that justice consists in a sovereign power of distributing boons and blows. Socrates accomplishes this disengagement by showing Polemarchus that such a conception diminishes the excellence of justice and calls into question the goodness of loyalty to friends (332c5–334b7). Second, Socrates elicits a new recognition on Polemarchus' part of the dearness of the unknown good.[16] This recognition results in a remarkable reorientation of Polemarchus' spiritedness. Socrates achieves this reorientation by showing Polemarchus that the true friend is the good, even when this good is not yet apparent as such (334b7–335a5). The acknowledgment of the dearness of the unknown good, I shall suggest, constitutes genuine piety or reverence. Because this acknowledgment is informed by a recognition of the *dearness* (as distinguished from a *putative knowledge*) of the good, it establishes in the soul a robust loyalty to the authority of the good, which is resistant to the venal or hubristic tendency to declare oneself instead as autonomous.[17] Third, and finally, Socrates argues that the good is never a cause of corruption or harm. It follows from this that the determination to do damage or harm to anyone must involve turning one's back on the good—a manifest act of disloyalty toward the true friend! Socrates thus demonstrates, in terms that *thymos* itself will find persuasive, that justice involves a steady orientation toward the good and so cannot involve the determination to harm anyone.

The Disengagement of Polemarchus' Spiritedness

Socrates now commences in earnest his examination of Polemarchus' conception of Simonides' understanding of justice. "By Zeus!" he demands, with some spiritedness now of his own, "What would Simonides say if someone asked. . . ?" (332c5–8). Judging from the thrust of Socrates' subsequent questioning, we may surmise that this oath represents a didactic expression of vexation at the feebleness of Polemarchus' present appreciation of justice. For Socrates immediately elicits Polemarchus' agreement that arts other than justice, each within its own domain, have the capacity to render what is fitting and due. Consequently, one is left to wonder just what, if anything, is rendered by the art of justice—and to whom (332d2–3). Polemarchus follows the line of questioning perfectly well. He maintains that justice consists in benefiting friends and damaging or harming enemies. But this is an unsatisfactory answer and does nothing to elevate the importance of justice, for the things fittingly assigned by other arts may also be understood, more generally, as goods and bads. When Socrates asks, "Who, then, has the most power to accomplish good for ailing friends and bad for enemies as regards disease and health?" Polemarchus sees the point. He accordingly ignores the claim of the just man and names the physician instead. Similarly, when the question is repeated, now concerning voyagers facing danger at sea, Polemarchus again ignores the righteous and instead nominates the helmsman as the one most able to render good and bad.

Searching, then, for the arena in which justice might come into its own, Socrates challenges Polemarchus to say where it is that the righteous will prove to have "the greatest power to benefit friends and to damage enemies" (332e3–4). Polemarchus answers that it is "in the conduct of war and in alliances" (332e5). But evidently not (as Socrates recalls on the basis of Polemarchus' previous admissions) when we are sick or at sea (332e6–10).[18] Polemarchus admits Socrates' point. Socrates then moves to broaden the perspective of the young "warlord," for Polemarchus so far has failed to mention the usefulness of justice in peacetime. Socrates alludes to this possibility and Polemarchus quickly responds that justice is indeed useful in peacetime, particularly in partnerships (332e11–333a14). But again, when Socrates invites Polemarchus to choose between the expert and the just man as the more useful and better partner in each of several transactions, his young interlocutor chooses the expert in every case. Polemarchus would prefer the collaboration of an expert draughtsplayer when positioning draughts, a mason when laying bricks, and a harpist when playing the harp. In all these cases we may say that Polemarchus rates virtuosity above virtue.

Yet there is one situation in which it is the just man who seems to Polemarchus to be the better partner—in partnerships where there is money dealing. But this turns out to be too broad an endorsement even for Polemarchus, for he admits, under pressure from Socrates, that if the partners are *to use* the money (say, to buy or to sell a horse in common), then someone who has technical knowledge (in this case, about horses) would be a better partner than the just man (333b11–c2). It is only when money is *not* put to use but merely needs to be kept safe that the just man is the better partner. Socrates summarizes the discussion, employing images that together represent the comprehensive human experience of war and peace: justice comes into its own "when there is need to guard shield and lyre and no need to use them; but when there is need to use them, it is the art of the infantryman and the musician that are of use" (333d6–9). Socrates thus presents the work of guarding or preservation as the least glorious, the least worthwhile, of human activities. Polemarchus is moved to agree that justice is useless for useful things and useful only for useless things. Socrates turns to his young interlocutor and remarks "Justice, then, my friend, would not be worth taking seriously at all" (333e1–2).

Socrates addresses Polemarchus as his friend (*ō phile*), while examining the notion that justice consists in benefiting friends and harming enemies. He thus invites us to ponder what good thing it is that he is accomplishing for this dear young man. Of course, many readers have doubted that Socrates is conveying any benefit at all. After all, Socrates has just led Polemarchus to a fallacious conclusion; his denigration of justice clearly exceeds anything required by the logic of the argument. We agree. Socrates' disparagement of justice is, in every sense of the word, gratuitous. Certainly, nothing in the work of guarding or preservation requires such a disparagement. If anything, the notion of justice as safeguard or preservative implies an enormously attractive role for this virtue, as subsequent discussions in the *Republic* will elaborate. *Justice Saves* is by no means a slogan incapable of stirring the blood. But Socrates plays this down for the present, precisely because it is his concern to settle, rather than to stir, Polemarchus' blood. In so doing, Socrates accomplishes something very much of benefit to his friend. And the benefit is indeed gratuitous, that is, a true gift. For, as Socrates notes, Polemarchus' definition of justice—benefiting friends and harming enemies—is typical of the frame of mind of "some rich man who supposes that he has the power to do big things" (336a6–7). (It was Polemarchus, we recall, who at first threatened to use force to compel Socrates to remain in the Piraeus. In that threat, Polemarchus placed himself beyond the influence of reason; he refused even to listen to Socrates' attempts at persuasion. The only thing to which he would give way was even greater power [327c9].) Dazzled by the power to accomplish "big things," a youth such as Polemarchus will grow oblivious to the need to learn *the good things*. If such a youth is ever to begin to consider what is good, he must first be disengaged from his attachment to what he currently regards as big or great. So we may say that in Polemarchus' case, at least, Socrates' familiar use of the analogy between virtue and the arts does not merely serve to emphasize the role of intellect in what is ordinarily called moral virtue. The prosaic implications of this analogy serve, even more importantly, as an antidote to the charm that power holds over our understanding of human excellence.

Socrates invites Polemarchus to consider further the expert craftsmen in whose likeness he has been modeling the just man. It is noteworthy that in the course of this consideration Socrates never speaks of this expert in the professional form of address. For example, instead of referring to the physician (*iatros*) as such (as Polemarchus had done, 332d12), Socrates speaks of "the one who is cleverest at guarding against illness as well as engendering it unnoticed" (*noson . . . phylaxasthai kai lathein houtos deinotatos empoiēsas*, 333e6–7). The circumlocution draws attention to the possibility that true professionalism, embodied in the physician properly so-called, is not to be identified with moral neutrality or mere dynamic bivalence. It must be admitted, however, that even if it is not morally neutral in its essential act, *technē* does remain peculiarly liable to exploitation for alarmingly divergent purposes. It is with a view to this liability, I would suggest, that Socrates introduces to the discussion the term *deinos*, which means "terribly (or, perhaps, terrifyingly) clever," in order to characterize the disposition of one who exploits the equivocal or bivalent potentialities of technical expertise. For the professional, like the clever man, plies a craft; but unlike the clever man, he never permits his craft to degenerate into mere craftiness. Socrates' strategy is to show Polemarchus that this attribute of cleverness or *deinotēs*, with which the lad's current conception of justice is contaminated, is not merely useless but is vicious in itself and repugnant to genuine friendship.

Socrates poses several leading questions and suggests a literary parallel, which together move Polemarchus to reject anything tainted by cleverness. Socrates induces Polemarchus to agree that (1) the most clever at striking a blow in battle is also the one to guard against such blows, (2) whoever is clever at guarding against illness is also most clever at causing illness unnoticed, and (3) the good camp guard is also the very man to steal what belongs to the enemy (333e3–334a4). Polemarchus is compelled by these admissions to conclude that the *deinos* guard—and so the just man, in accordance with his current conception of virtue—is essentially a clever thief (334a5–10)! Socrates then blames Homer for teaching Polemarchus this lesson. Homer, Socrates notes, is especially fond of Odysseus' grandfather, a man named Autolykos (roughly, "Lone Wolf"). In the words of the poet, this Lone Wolf "surpassed all mankind in thievery and in oath-swearing" (334b2–3; cf. *Odyssey* 19.394–95). What it means to surpass all mankind in thievery is plain enough, but how one wins such distinction "in oath-swearing" calls for an explanation. Stanford surmises that Autolykos excelled all mankind "by cleverly framing his oaths so as to leave loopholes for advantageous evasions later" (1958, 2:332). As a devotee of Hermes (from whom our word *hermeneutics* derives), Autolykos was able to exploit verbal equivocation and so was able always to get the better end of promises and bargains (*Odyssey* 19.396–98). Thus, in view of Polemarchus' preceding admissions concerning the bivalent potentialities of technical expertise and so of the righteous man himself, Socrates quite reasonably offers Homer's praise of Autolykos as a fitting epigram of Polemarchus' appreciation of justice.

But if the elevation of Autolykean cleverness to the status of righteousness is what his argument comes to, Polemarchus wants no part of it. "No, by Zeus," he swears, "though I, at least, no longer know what it is I meant" (334b7). This vehement rejection of Socrates' imputation indicates that Polemarchus would prefer to renounce his own claim to know what justice is rather than to endorse as just the cleverness characteristic of Autolykos. Surprisingly, Polemarchus is unmoved even by Socrates' stipulation that such cleverness be employed "to benefit friends and to harm enemies" (334b3–5). One might have expected this stipulation to have made all the difference to Polemarchus. There can be only one reason why it does not. The practice of Autolykean cleverness must itself be at odds with the good of friendly solidarity. Polemarchus evidently notices that Autolykos, the Lone Wolf, remains essentially a loner regardless of who happens to benefit from his cleverness. Socrates' delicacy of expression has paid off; Polemarchus has come to see the terror signified by the term *deinos*. Indifference to the good, wherever it surfaces, makes us strangers even to those who are otherwise dearest to us.

Of course, in defense of cleverness, someone might assert that the versatility of *technē* implies the absence of a *telos* or goal proper to the art as art and so authorizes the clever man's willingness to ply his craft for evil as well as good ends. But this is mistaken. The versatility of the arts—which indeed renders them liable to such exploitation—simply reflects the importance of what the Greeks called *kairos* (the due measure, appropriate instrument, opportune time, etc.) in human affairs. The possession of *technē* entails an ability to address the exigencies of any occasion. Owing to the diversity of circumstances, an action that at one time promotes a particular purpose will at another time thwart it. There is, therefore, a necessary versatility in the capacity that the technical expert commands. For example, a physician will be equipped both to feed (a cold) and to starve (a fever); this versatility is properly exercised in the interest of promoting the intrinsic end of medicine, namely, health. The clever man exploits this versatility, however, not in pursuit of health but rather to further some extrinsic end. For example, "the one who is cleverest at guarding against illness as well as engendering it unnoticed" might take steps to starve his political opponent and to feed his favored candidate. Such an outcome might be thought to be attractive to a man like Polemarchus, who takes devotion to friends so seriously. But there is no reason to suppose that someone who is indifferent to the good of his own art is going to be any more attentive to the good upon which his "friendships" can be distinguished from merely arbitrary or chance associations. Polemarchus rejects cleverness, then, because he senses that it is at odds with the foundation of genuine friendship even though Socrates has stipulated that such cleverness is to be employed for the benefit of friends and the harm of enemies.

Moreover, at the same time that he heartily repudiates cleverness, Polemarchus reveals his new openness to self-examination. "No, by Zeus," he declares, "though I, at least, no longer know what it is I meant; yet it still seems to me that justice consists of benefiting friends and damaging enemies" (334b7–9). We may say that this remarkable statement, taken as a whole, indicates (1) that Polemarchus now recognizes that his opinion is an opinion, not knowledge; (2) that he is astonished at this recognition and so has genuinely learned something in coming to it; and (3) that his *thymos* is now oriented toward upholding or enforcing this recognition.

Polemarchus Acknowledges the Dearness of an Unknown Good

Polemarchus knows now that he does not know what justice is. He admits, nevertheless, that justice still seems (*dokei*) to him to consist in benefiting friends and harming enemies. So *Polemarchus* has changed, even if what seems or appears to him has remained the same; he is now in a position to examine rather than merely to insist upon this opinion. Measured by Socratic standards, this is a most significant development. Socrates' response is to launch Polemarchus upon the examination of this opinion. He asks whether Polemarchus means by friends "those who seem to each to be good or those who really are good, whether they seem to be or not" (334c1–3). Polemarchus conjectures that one "befriends those whom he believes [*hēgētai*] to be good" (334c4). Polemarchus' word *hēgētai* implies rather more than a mere perception, which could be derivative from preexisting affinities: *hēgētai* designates instead a *leading* belief, thus compactly conveying Polemarchus' conjecture that it is one's belief concerning the good that is the *hegemonic* factor in determining his affinities, rather than vice versa. Such a hegemony is in fact essential to the independence of *thymos*, to its very existence as something more than the love of one's own. For if our belief concerning the good were exclusively beholden to what is nearest and dearest, our capacity for friendship could never transcend our cultural circumstances. Were this true, a social science informed by the doctrine of cultural relativism could eradicate any need for philosophy. But a truly hegemonic belief concerning the good, the only kind of belief worthy of *thymos*, cannot take its marching orders from what is currently familiar. Instead, as we shall see, it must be informed by an acknowledgment of the dearness of an *unknown* good.

Socrates next asks Polemarchus whether human beings err in their beliefs concerning the good, such that in their view many seem good who really are not good and many others do not seem good who really are (334c6–8). Polemarchus acknowledges that human beings do err in this way (*hamartanousin* 334c9). Socrates notes that it would then be just, on Polemarchus' account of justice, to harm the innocent. But there is no way (*mēdamōs* 334d7), Polemarchus declares, that he will brook such an enormity. Rather than affirm so transparent an injustice, Polemarchus vehemently denounces as burdensome and wicked (*ponēros* 334d7–8) any account of friendship, including his own, that is open to such an implication. In the case of a conflict, Polemarchus will side with what is right, rather than with what is his own. Granting that it is not right to harm the innocent, Socrates still wonders what is right. So he inquires, "Is it right, then, to harm the unjust and to benefit the just?" (334d9–10). Polemarchus answers that this appears to be nobler (*kalliōn*) than the converse. But, as Socrates proceeds to point out, this new formulation renders friendship completely irrelevant to the consideration of justice, contrary to what Simonides had said (334d12–e4). In other words, Socrates appeals to the authority of Simonides here in order to remind Polemarchus that one cannot utterly disregard friendship in the course of doing right. Polemarchus' task is to uphold the relevance of friendly solidarity in the consideration of justice without permitting it to become a source of corruption. Polemarchus sees perfectly well, now, what must be done; he must qualify the role of perception in the determination of what is dear. For the sake of friendship, Polemarchus is prepared to move beyond the limits of perception. He urges Socrates to join him in changing (*metathōmetha* 334e5) what they had posited as the friend.

Socrates and Polemarchus had been supposing that it is the seeming good that is dear. Polemarchus proposes that they now define as *philon* that which both seems and really is good (*Ton dokounta te . . . kai ton onta chrēston,* 334e10). He infers that the apparent good—if it is not also really good—is only apparently, though not really, dear (334e10–335a2). By the same token, we may say that the really good—if it is not also apparently good—is really, though not apparently, dear. Socrates seizes the latter inference, noting that "on this account the good will be dear, as is fitting" (335a3). Socrates thus indicates that Polemarchus' new position brings to light the immediacy of the connection between what is really good and what is really dear. What is fitting has, on this basis, also become apparent: namely, that one's acknowledgment of what is dear is not exclusively a function of current perception. Following Socrates, we may suggest that the most important consequence of Polemarchus' reformulation of the definition of the *philon* is not its obvious indication that the good must be perceived for it to appear to be dear but rather its subtle implication of the real dearness of a good that is not yet apparent as such. It is on this basis, we may surmise, that Socrates now speaks of a good that is not simply *chrēston* but *agathos* (335a3, cf. 334c4, 334e10). The verbal distinction—the contrast between these terms—indicates that the *true friend* is a good distinctively beyond what we presently recognize as useful (*chreia*) to us.

This insight permits Polemarchus to uphold the relevance of friendly solidarity to justice and, at the same time to resist its decomposition into nepotism. Polemarchus thus succeeds because he has found a true friend in the good. Put somewhat differently, Polemarchus finds that if he is to honor friendship as he deems fit, he must attend more to the existence of a sovereign good than to his own possession of a sovereign power. This finding, I suggest, marks the dawn of genuine piety or reverence in the soul of Polemarchus. Polemarchus' devotion to the cause of friendship does not require that he know the good, only that he acknowledge the dearness of the good he does not know. Now, this discovery by no means implies that Polemarchus does not have *more* to learn. In fact, he does, for in his first attempt to demonstrate his friendly solidarity with what is *really* good, Polemarchus agrees with the proposition that "it is just to benefit the friend precisely because he really is good [*agathon onta*], and to harm the enemy precisely because he really is bad [*kakon onta*]" (335a9–10). Thus it still remains for Polemarchus to learn that the determination to harm *anyone* will tend to alienate him from the true friend, rather than drawing him closer to it.

Polemarchus Learns That the Righteous Do No Harm

As matters stand, Polemarchus supports what may be called the homeopathic conception of justice—that it is right to do good things for the friend (who is good) and to do bad things to the enemy (who is bad). His recollection of the dearness of the true good has rekindled his commitment to uphold and to honor above all the distinction between friend and foe. Because it parallels this distinction by rendering good to the good and bad to the bad, the homeopathic conception of justice appears to Polemarchus to be elegantly or beautifully formulated (*kalōs legesthai* 335b1). But Socrates wonders if it is the part of a just man to harm any human being whatsoever (335b2–3). Polemarchus replies, "Well, of course. One is obliged [*dei*] to harm those, anyway, who are both wicked and enemies" (335b4–5). Polemarchus believes that the righteous are *bound* to harm the truly wicked, for if they were not to do so they would fail to uphold the distinctiveness of the good. Polemarchus supposes that we demonstrate our allegiance to the good by treating those who are good in as different a way as possible from those who are bad. To shrink from doing bad things to bad men seems a failure of nerve in distinguishing the good from the bad.

It is reasonable of Polemarchus to insist upon the importance of maintaining distinctions. There is indeed something disorderly, shameful, and even ugly in the failure to make proper distinctions, especially distinctions between good and evil. Moreover, we may grant that good and evil are opposites, as are benefit and harm. But it is at this point that we must reconsider the nobility and the reasonableness of moral homeopathy. Is moral homeopathy truly *kalon* —is it even successful—in upholding, and illuminating, the fundamental opposition of good and evil? Or is it the part of a just man never to harm any human being whatsoever? Now Socrates may well agree with Polemarchus that the noble or beautiful somehow glorifies and illuminates the distinctiveness of the good. But he clearly departs from Polemarchus' view by denying that the *kalon* consists in anything like a homeopathic operation. Socrates argues, for example, that the result will not be *kalon* if a statue painter assigns the finest color, purple, to the finest part of the body, the eyes (420c4–d5). If the statue painter were to guide his actions by this homeopathic pattern, it would turn out that the statue's eyes "do not show themselves as eyes at all." Though we might commend the sincerity of this statue painter, who simply wishes to honor what is fine by anointing the finest part of the body with the finest color, we must make no mistake about the seriousness of his shortcoming, for his homeopathic aesthetic obscures rather than illuminates the eye—the very organ in which beauty and nobility in the body is most evident.

Polemarchus, along with the statue painter of Socrates' example, may be faulted for committing the error of *moralism*. Moralism asserts itself in a certain preoccupation with rewarding good and punishing evil, which causes one to pay insufficient regard to the ontological significance of the thing that is subject to these judgments. So when Socrates asks whether it is the part of a just man to harm or to do damage to any human being whatsoever (*hontinoun anthrōpōn*), it happens that Polemarchus omits any mention of *anthropos*—of human being—in his affirmative response (335b2–5). Polemarchus, of course, believes that a homeopathic assignment of benefit and harm is the just and ordinate response to the presence of good and bad. But the grammatical ellipsis indicates that Polemarchus does not properly appreciate the ontological implications of such an undertaking. Polemarchus has not yet faced up to the fact that it is a human being, and not evil itself, that is damaged by homeopathic justice.

As Socrates hastens to point out, however, it is a real being who is made worse when real harm is done; this worsening of a real being involves a diminution in virtue or excellence (*aretēn* 335b6–c3). Polemarchus proves to be receptive to Socrates' reminder of the general ontological significance of doing harm: "That must be," he replies (335b12). Socrates now addresses Polemarchus as his comrade or collaborator (*ō hetaire* 335c1). Together they maintain that when human beings are harmed it is as human beings (i.e., in respect of human excellence) that they are diminished. The comrades dismiss other notions of harm, which are, of course, conceivable but are nevertheless not telling in the present instance, because such notions fail to reach the ontological underpinnings of right and wrong. In commencing a search for these underpinnings, we must remember that the deprivation of contingent goods may not constitute a harm at all. Socrates later notes, for example, that sickliness actually proved to be a boon to another of his comrades, Theages (496b6–c3). Owing to their circumstantial nature, the presence or absence of such contingent goods in any given pattern of distribution does not truly shed light upon the *fundamental opposition* between good and bad. But Polemarchus believes that it pertains to justice to uphold this opposition. So justice cannot be a merely contingent human good, it *must* be essential to what we mean by human excellence (335c4–5).[19] Once Polemarchus has acknowledged this, he leaves the distribution of boons and blows to others and joins Socrates in the search for this essential, and not merely contingent, human excellence.

Polemarchus' rejection of cleverness has by now radically altered his conception of art. Thus, we find in the present passage that he understands musicianship and horsemanship as professional qualities rather than as exploitable crafts or powers (335c9–13). Polemarchus has learned to concentrate on the essential act of a *technē* as distinguished from its divergent potentialities.[20] He no longer desires to acquire such an equivocal *dynamis*, for he has come to understand that friendly solidarity is in no way enhanced by the exercise of power in this sense. To be loyal to the true friend, which is the good, one must uphold its fundamental opposition to what is bad. But one can uphold this opposition only by admitting that it is impossible for justice to cause harm or damage to any human being. Justice cannot consist in a power whereby one even alternates good and bad. If justice could ever cause harm, then it would follow that the essential human excellence could cause its own diminution. In that case, the human good would cause the human bad. But if the good were *ever* to cause the bad, good and bad would be only conditionally and not fundamentally opposed to one another. In order to guard against this implication, Polemarchus embraces without the slightest reservation the proposition that justice never works harm (335d13).

It follows that if goodness never causes harm, one can do harm only by parting or turning oneself away from the good. Polemarchus' thymotic passion for friendly solidarity (and thus for upholding the opposition between good and bad) can only be satisfied, then, if he is himself turned steadily toward the good. Informed by this insight, Polemarchus' spiritedness will henceforth become the perfect ally of philosophy, permitting nothing to distract him from the good. Spiritedness must be so allied in order to escape distraction by the juvenile desire "to settle a score." Socrates will later allude to the undistracted attention to the good as the greatest study (*to megiston mathēma*) and the one most appropriate for the philosopher (504e4–505b4). So we may infer that only philosophy will satisfy and correctly order the thymotic longings of Polemarchus' soul. But we must also note that philosophy can only be pursued if one first recants the childish notion that it is ever just to harm anyone.

Socrates finally invites Polemarchus to become his partner in defense of Simonides (and other wise and blessed men) by fighting anyone who holds that such men deem it right to harm anyone (335e7–10). Although the argument indicates that Simonides could not have meant that it is right to harm anyone, it has of course not settled what he *did* mean. Socrates and Polemarchus thus become partners in defense of a good they do not themselves possess or know. Their acknowledgment of the dearness of this unknown good is not only evidence of Polemarchus' piety, it also comports perfectly with the continuing conduct of philosophical inquiry. Plato will not wait long before giving his reader an example of the Socratic–Polemarchean partnership in combat, in which the harmony of piety and philosophy is made perfectly clear.

POLEMARCHUS' CONTRIBUTION TO THE REMAINDER OF THE DIALOGUE

Considering Polemarchus' ripening into a representative of true piety, it is only appropriate that his important contributions to the subsequent movement of the dialogue are conveyed in a most modest and quiet manner. We should take notice of these contributions by way of completing our discussion of his role in the *Republic*.

Polemarchus appears twice more in the dialogue. We hear from him again very soon, in the course of Socrates' conversation with Thrasymachus. In the midst of the first half of this conversation, Plato introduces a brief interlude in which Polemarchus confronts his dramatic foil, Cleitophon. The point of this confrontation, I suggest, is to show the reader that Polemarchus is not at all like Cleitophon. One might say that the confrontation between Cleitophon and Polemarchus in this passage supplies the best index of Socrates' success in his pedagogical undertaking with Polemarchus;[21] for rather than turning out to be like Cleitophon, Polemarchus proves to be diametrically opposed to him. Yet the comparison of the two young men is inevitable. They both represent tendencies characteristic of the broader political community. Polemarchus, we recall, attempts to impose the force of democratic numbers in the dialogue's opening enactment of the arrest of the philosopher (327c7–14). And Cleitophon advocates the purest form of legal positivism proposed in the dialogue. Cleitophon goes further in this respect than even Thrasymachus; for Cleitophon holds that when Thrasymachus says that might makes right, what he means to say is that right is anything the politically dominant class *believes* to be to its own advantage (340b6–8). But this clever loophole, which could have insulated Thrasymachus' position from Socrates' refutation, would also make philosophical inquiry impossible and unnecessary. Philosophy is possible or necessary only if one first recognizes the importance—nay, the dearness—of what he does not know. But there is clearly no room for such piety in Cleitophon's world. Cleitophon's perceptionism enthrones every man as measure and absolute ruler of his own private cosmos. Cleitophon's anxiety to escape refutation leads him to annihilate the possibility of recognizing his own ignorance and, along with it, the possibility of ever learning anything. But Polemarchus, in the course of his conversation with Socrates, has come to appreciate the dearness of the unknown good. He could only do this, as we have seen, by first recognizing the limitations of perception, the inadequacy of the seeming good. So it is inevitable that Polemarchus will find himself at loggerheads with Cleitophon, for whom the seeming good is a jealous god. Yet it does no harm to Cleitophon—and in fact it positively benefits Thrasymachus—when Polemarchus testifies that Thrasymachus did not say that justice is whatever seems to be advantageous to the mighty (340a1–b9). By speaking out and revealing his own sympathies, Polemarchus makes it more difficult for the crowd-pleasing rhetorician to adopt Cleitophon's loophole. In this sense, Polemarchus plays an important role in making it possible for Thrasymachus and Socrates eventually to become friends. Had Thrasymachus followed Cleitophon's impious tack, undermining the very possibility of philosophy, he could never have become Socrates' friend and might very well be regarded as his enemy (498c6–d1). This, then, is how Polemarchus first answers Socrates' call to do battle in partnership with him. He is Socrates' true ally in upholding the possibility of philosophy and in waging a battle from which everyone might benefit.

The next and last time we hear of Polemarchus in the *Republic* is at the opening of book 5. His contribution to the conversation in this passage is muted but is, nevertheless, enormously significant, for it is with hardly more than a whisper that Polemarchus moves Adeimantus and the entire inquiry out of the theoretical doldrums. Taking his cue from Polemarchus, Adeimantus articulates their common concern (*hemeis* 449d1, *tithete* 450a4), speaking at considerable length of Socrates' faintheartedness (*aporrhathymein* 449c2) and injustice in "stealing a whole *eidos* of the account" of the city-in-speech. So it is Polemarchus who propels Adeimantus and the others to put a stop to Socrates' fainthearted speculation concerning the forms of bad constitution. As a consequence, Socrates is compelled to return to the more arduous task of completing the articulation of his model city. From this nearly inaudible yet decisive intervention of Polemarchus there develops the "digression" of books 5–7, in which Socrates and his interlocutors renew their pursuit of philosophy and the good with an unprecedented seriousness.

CONCLUSION

Socrates goes down into Piraeus with Glaucon both to pray and at the same time to see. I have suggested that the desire to reconcile reason and piety, which is manifest in this twofold intention, is a just and fitting response to what is perhaps the most vital feature of the human condition. As Socrates maintains, human beings are contingent creatures who nevertheless possess the ability to share somehow in the noetic activity characteristic of the divine. I have acknowledged that the departure of Cephalus, who abandons inquiry in order to attend to certain sacred rites, induces us to consider the tension between piety and reason. But I have also suggested that the textual passage centered on this focal event reveals that it is not Cephalus but Polemarchus who, together with Socrates, deserves to be regarded as the dialogue's exemplar of piety.

The textual evidence suggests that Socrates intends to unmask Cephalus as a representative of rationalism rather than piety. Following this evidence, I maintain that we must reject the widespread notion that Cephalus' departure constitutes Plato's dramatic indication of the fundamental incompatibility of philosophy and piety. Instead, we would do better to conclude that Cephalus' departure dramatizes the incompatibility of Socratic philosophy and rationalism. Because rationalism is also hostile to genuine piety, Cephalus must take his leave before true piety can be introduced and reconciled with Socratic philosophy.

I have argued that it is Polemarchus whose development dramatizes the character of genuine piety. Polemarchus enters into the conversation advocating a blind faith in the authority of the wise man, Simonides, as a basis upon which to decide the question of justice. But Socrates ripens and fortifies this piety by bidding Polemarchus to consider "what it is you say that Simonides said correctly in speaking about justice." It might seem that such a question could not be seriously intended to initiate a philosophical inquiry because it takes for granted that in some sense Simonides *is* right. This assumption, I agree, is by no means beyond all possible doubt. But the analysis I have offered suggests that it is precisely the piety—or "bias," if one insists—of this form of question that underlies the love of wisdom. Piety, thus understood, keeps philosophical inquiry steadily concentrated upon the good, undistracted by other considerations.

I have suggested, by the conduct of this inquiry, that one can best approach the study of Plato in the same way. In order to learn from even the closest study of small passages of Plato, the reader must hold dear what he does not know. The reader must be supremely attentive (we might even say reverential) toward the text. This requires respect for the text's structural articulation and for the dramatic context of every argument. With this reverential approach, one recognizes that logical ambiguity may serve as an impeccable conveyance of significance; that silences may speak eloquently; and that blunders that "would shame an intelligent high school boy" may become for the attentive reader critical pointers to an underlying, coherent teaching. The authority of logic is not thereby rejected. Instead, the reader assumes the responsibility to remain alert enough to supply the suppressed premise of an enthymeme. Piecemeal logical analysis is thus transcended in the direction of the holistic consideration of logographic necessity. Such a transcendence sustains the virtues of reason as against the shortsightedness of rationalism. In this way, moreover, the student of Plato adopts Socrates' recommendation to become both judge and advocate at the same time. On the other hand, I would suggest that the more perfectly neutral inquiry "Is Plato right or wrong?" will inevitably distract one's inquiry from the good merely in order to settle a score with an author. Instead of serving as a portal to the good, the author becomes in this case the principal object (and hence, inevitably, an obstacle) to what was intended as a philosophical investigation.

I have argued that Polemarchus' spiritedness is initially inclined toward such score settling. Polemarchus passionately strives to achieve the most rigorous possible discrimination between friend and foe and thus hits upon the notion that to do right one must benefit friends and harm enemies. This notion in turn tends to provoke a dreadful appetite for the power to dispense benefit and harm. Socrates opposes this appetite, first, by disengaging Polemarchus' enthusiasm for this notion of justice. He does this by showing that its practice implies a status for justice that is inglorious and which, moreover, casts doubt upon e goodness of friendly solidarity. Socrates then helps Polemarchus to identify the proper object of his heart's desire in the true good, even when the good is not yet apparent as such. I maintain that Polemarchus' acknowledgment of the dearness of the unknown good constitutes the definitive indication of genuine piety in the conversations of book 1. Because only the disposition to hold dear what one does not yet know can keep one mindful of the need to learn, we may say that piety in this sense is indeed a docility of soul, not in the mistaken current sense of intellectual passivity but in the original sense of *docilitas*, or "learning readiness." Such docility, moreover, is the polar opposite of the hubristic resistance to learning, or *amathia*, which is the culprit so often obstructing philosophical inquiry in Plato's dialogues. With Polemarchus' recognition of the dearness of the unknown good, his spiritedness is set squarely to the task of upholding the opposition between good and bad. It remains only for Socrates to persuade Polemarchus that this opposition cannot be maintained unless justice is oriented steadily toward the good; as a consequence, Polemarchus learns that the just man cannot seek to harm anyone.

Thanks to Socrates' examination of Polemarchus, we are better able to appreciate the character of piety or reverence. Typically, this quality is mistaken as a merely submissive or passive deference to higher authority. But it is more properly understood as an acknowledgment of the dearness of (and hence as a receptivity to) the unknown good. Such receptiveness is manifest not in blind faith but in a questioning that is searching even as it anticipates the truth or goodness of the thing questioned. Piety in this sense is characteristic of one who is at the same time both judge and advocate, and yet neither skeptic nor dogmatist. I thus conclude that it is Socratic piety (and not its diametric opposite, Cartesian doubt) that is the disposition most congenial to the love of wisdom. Piety, we may say, is not merely necessary to thwart the persecution of philosophers. Piety is essential to the pursuit of philosophy.

Notes

1. Citations in this essay to the *Platonis Opera* (Oxford, 1900–1907) supply the standard Stephanus divisions of page, section, and line. Unless indicated otherwise, all references are to the *Republic*. The translations are my own.

2. Of course, to say that this understanding is *accessible* is not to say that any particular student will in fact gain access to it; but it is good pedagogical practice never to lose sight of this possibility.

3. See also Strauss 1965, 1–31 idem 1979.

4. As it happens, Stephanus divides this passage into 44 sections (327a–336a), with Cephalus making his departure in the last line (331d9) of the twenty second section. The eidetic standing of this collection of 44 sections may be better appreciated if one notes that it already comprises a sufficiently diverse assemblage of interlocutors to reflect something of the character of the *Republic* as a whole. Cephalus, Polemarchus, and Socrates may be said to correspond to the three classes (money-maker, warrior, and philosopher) of Socrates' model city and thus may be said to anticipate the problem of the wholeness of its *politeia* and perhaps of *politeiai* in general. Euben is right to observe that the triplet of Cephalus, Polemarchus, and Thrasymachus does *not* correspond to the threefold structure of the model city (1990, 248). Thrasymachus, after all, is not a philosopher. Of course, neither Euben's observation nor my claim concerning the significance of the passage upon which I propose to focus is meant to imply that Thrasymachus is unimportant. On the contrary, I would suggest that his presence in book 1 poses the important problem of philosophy's need to incorporate rhetoric while at the same time separating itself from sophistry. But one cannot talk of everything at once: if we try, we shall find ourselves impaled upon the "forest" horn of the forester's dilemma and reduced to the formulation of cocktail-party generalizations. The challenge is to find a way to think and talk about the details nonreductively, that is, while preserving their relationship to the whole.

5. I am thus inclined (with Brann 1967, 88, 113) to view the narration of the *Republic* as an example of the Pythagoreanism of Plato's Socrates. Pythagoreans sought to cultivate a capacity for just this sort of recollection. According to the testimony of Iamblichus, "a Pythagorean man does not arise from his bed before he has recollected what happened yesterday" (*Life of Pythagoras* 163.20, cited in Brann 1967, 113). This recollection would be a private exercise. Hence, Socrates' narration of Plato's *Republic* may be likened to a dramatic soliloquy. Nevertheless, one might wish to maintain that Socrates does have a circle of auditors who listen to his rehearsal of the conversation of the *Republic*, as seems suggested by a possible (though by no means necessary) interpretation of the opening scene of the *Timaeus*. There is, however, no evidence internal to the *Republic* to support such an interpretation.

6. An example from the present passage will suffice to illustrate this point. Socrates expresses personal admiration (*egō agastheis*) for Cephalus in a narrative gloss, when the latter reckons that a good way of life is more important to personal happiness than the capacity to enjoy sexual pleasures (329d7). I take it, then, that we may understand this admiration as sincere, if provisional. On the other hand, it is in direct discourse with Cephalus himself that Socrates describes as *pangkalōs* (i.e., beautiful in every respect) Cephalus' view that money is chiefly useful in making it unnecessary to commit injustice (331c1). Hence, I am inclined to take this evaluation with the proverbial grain of salt. In fact, Socrates' pretentiously exhaustive word *pangkalōs* might be understood as insinuating that in some decisive respect Cephalus' notion is quite the opposite of beautiful. The sequel, as we shall see, confirms this insinuation. Such is the character of Socratic irony.

7. Socrates employs this *te . . . kai hama* formulation throughout the *Republic* to indicate the pairing of things that might well be regarded as irreconcilable. He remarks that the discovery of such a pairing naturally awakens dialectical thought (524e2–5). Sometimes, however, the "longer path" of dialectic is postponed and a provisional understanding is accepted merely by hypothesis (as indicated, for example, at 436d4–437a9). For a discussion of the pedagogical significance of this postponement of dialectic and its importance for the interpretation of the political institutions of Socrates' model city, see Dobbs 1994.

8. The case that Socrates' conventional religious practice is more than window dressing is made by Lloyd-Jones, among others, who notes that in late fifth-century Athens many educated people realized "that the traditional beliefs and observances were by no means incompatible with a more sophisticated kind of theism; belief in a god or gods that had been defined by the doctrines of philosophy could easily go together with the maintenance of the ancient worship" (1983, 134).

9. Adam points out that in Attic literature the phrase *the goddess* regularly denotes Athena (1902, 1:62). Nevertheless, he rightly considers evidence internal to the *Republic* to be more compelling than regular Attic usage in interpreting this phrase; this internal evidence suggests an identification of Socrates' goddess with Bendis. I agree with this assessment,

but venture further to say that it would be mistaken to substitute terms, reading *Bendis* instead of *the goddess* in hope of improving upon Socrates' use of an ambiguous phrase. It is more prudent to keep open the possibility that the ambiguity implicit in Socrates' speaking of "the goddess" is meaningful precisely as such. While the future participle *proseuxomenos* plainly indicates Socrates' intention to pray, his ambiguous diction in the case of the intended recipient of these prayers (Socrates himself never uses the name Bendis) indicate that he does not mean to pray to Bendis qua Bendis. Such ambiguity may be the most economical means of conveying the complexity of Socrates' recognition of this cult as reflecting an aspect of some unknown, though necessary, god. This would also explain why it appears that "Plato apparently regards the content of prayers as more important than to whom they are addressed (Jackson 1971, 34).

10. Of course, some readers are inclined to dismiss Socrates' testimony as ironic. But a careful review of the text will find no warrant for such a dismissal in this case. In the passage typically cited as evidence of his ironic intent, Socrates is commenting on the difficulty of persuading his fellow citizens of the importance of persisting in his cross-examinations. "If I say that it is impossible for me to keep quiet because this is to disobey the god, you will not be persuaded by me, on grounds that I am being ironic" (*hōs eirōneuomenōi*, 37e5–38a1). It should be noted, however, that the *hōs*-plus-participle construction, which Socrates employs in the final clause of this sentence, is used in Greek to give the reason or purpose of someone other than the speaker *without implying* that it is also the speaker's understanding of the situation (see Goodwin, *Greek Grammar* §1574). Hence, there is no reason to construe Socrates' statement as an admission that he is being ironic; his statement merely affirms that others will suppose that he is. Perhaps many in the democratic jury will doubt Socrates' veracity in this matter because they are not themselves inclined to take the oracle at Delphi (or the voice of any other *daimōn*) very seriously but confide instead exclusively in their own autonomous will. So it happens that Meletus, the poet, succeeds in his indictment of impiety against Socrates only with the support of Anytus, the democratic politician. The democracy naturally treats complicity in the rearing of oligarchs as the functional equivalent of impiety.

11. Morgan makes an intriguing point, noting that inasmuch as he is a metic, Cephalus resembles the human soul, which also is a sojourner "living temporarily in the world of history, politics and moral conduct, . . . always anticipating its future and the possibility of permanent citizenship among the divine" (1990, 100). I think this suggestion is even more pertinent to Cephalus' son, Polemarchus, who is also a metic but whose anticipation of the destiny of his soul is not marred by his father's anxieties.

12. See n. 6.

13. The term *rationalism* is of course a modern coinage. Ancient authors described this tendency in a more frankly pejorative manner, as a manifestation of hybris or recklessness. Such value-laden language testifies to the clarity of the divergence, in the view of these authors, between what we call rationalism and the true virtue of reasonableness. Dobbs (1987) and Saxonhouse (1988) may be consulted for further discussion of the ancient appreciation of the discrepancy between the virtue of reasonableness and the recklessness of rationalism. It is my intention here to present Socrates as a positive case in point, for Socrates' examination of Polemarchus, which is remarkably deferential to the authority of Simonides, remains a model of reasonableness, though it certainly would not pass Cartesian muster. As I shall suggest, Socrates' conversation with Polemarchus thus reveals that piety is more congenial to philosophy than is radical doubt.

14. In his attempt to argue that the sacred is not a concern for the philosopher but only for the city, Benardete fails to take account of the significance of this linkage of advocacy and criticism (1989, 82–83). Specifically, he faults Glaucon (who recalls Socrates' acknowledgment of a "sacred task to help justice," 368b7–c2, 427e1–2) for failing to appreciate the distinction between searching for justice and helping justi But it is reasonable for Glaucon to combine these concerns; by yoking the tasks of searching and helping, he faithfully executes Socrates' intention that they be "judges and advocates at the same time" (348b3–4). Although there is much to admire in Benardete's book, I maintain that we will never adequately appreciate the philosophical significance of Socrates' undertaking in the *Republic* until we acknowledge the importance of his pairing of piety and reason, advocacy and criticism.

15. *Thymos*, as Aristotle explains it, is the power of soul whereby we love or hold something dear (*philoumen*); it is indomitable and prerequisite to a capacity for rule and for independence (see *Politics* 1327b18–1328a16; cf. Plato, *Republic* 374a12–b2). Zuckert writes pithily that "Socrates identified *thymos*, or spiritedness, as the psychic origin of distinctively political action. . . . [In Socrates view,] political order does not arise directly from one's own desires or [even from] the means best calculated to realize those desires but rather in reaction to the excessive desires of others" (1988, 2–4). Socrates treats *thymos* thematically, though incompletely, in book 4 of the *Republic* (437a4–10, 439e2–441d4). An adequate consideration of the classic understanding of *thymos*, the seat of the emotions of love, anger, and indignation (to say nothing of the spirit of independence and self-sacrifice), would require a careful study of these and other passages.

16. Strauss notices that Hamlet's famous soliloquy illustrates the tendency of *thymos* to shift from "the justified indignation about injustice . . . into the unjustified indignation about unrequited love. This is perhaps the deepest secret of spiritedness and therefore at least one of the deepest secrets of the *Republic*" (1989, 168). To this remarkably incisive comment I would like merely to append the observation that in some sense the opposite of unrequited love is the discovery of an unknown friend. Such a discovery, which Polemarchus is about to make with Socrates' assistance, will naturally counter the thymotic tendency toward unjustified indignation. Moderation is thus nourished by Socratic piety.

17. Piety in this sense is utterly alien to the conduct of Euthyphro, the pseudospokesman for piety in the Platonic dialogue named after him, for Euthyphro plunders his cultic expertise to claim for himself the very prerogatives of divinity (see Strauss 1989, 197–98). Readers interested in the relationship between the *Euthyphro* and the *Republic* should also observe that Euthyphro's final definition (that piety consists in commercial transactions with the gods, 14c4–15b3) would lend credence to Cephalus' decadent piety. Socrates' refutation of this final definition, then, would likewise undermine Cephalus' position. But Socrates' refutation, which annuls Euthyphro's designation of self-serving commerce as the attendance (*therapeia*) that is due the gods, leaves open the possibility that some other relationship of attendance—noncommercial but as yet unspecified—might well constitute the true character of piety (see *Euthyphro* 13a1–2). It seems to me that Polemarchus' acknowledgment of the dearness of the unknown good qualifies as this other kind of attendance. Thus, the *Euthyphro* may be said to culminate in the refutation of Cephalus and the anticipation of Polemarchus. We may expect, then, that the *Republic* will involve a further elaboration of the consideration of piety initiated by Plato in the *Euthyphro*.

18. Justice may yet regain its prominence in the case that one is both sick and at sea at the same time, for then an appeal would have to be made to some third man beyond the physician and the helmsman to determine which of their possibly conflicting prescriptions to follow. Because of the familiarity with seasickness typical of a seafaring people like the Greeks, we may surmise that this implication is not entirely foreign to Socrates' intention in the choice of his examples.

19. Failure to consider this point has led many readers to the erroneous conclusion that it is foolish of Polemarchus to suppose that justice exhausts what we mean by human excellence. Of course, certain conceptions of justice are less than exhaustive of human excellence; some conceptions are

even at odds with it. Justice understood as the imposition of arithmetic equality in external goods, for example, might well subvert the excellent endeavors of some of the more talented members of society. But such conceptions of justice are blunted by the incongruity of their concern for contingent goods and so cannot penetrate to the depth that Socrates' present argument requires. Polemarchus, then, follows the train of Socrates' argument better than many of his critics, for once one is required to consider the issue ontologically, it may be necessary as well (*kai tout' anangkē* 335c5), as Polemarchus agrees, to accept righteousness as the good essential to human being. Moreover, if it is true that the central books of the *Republic* vindicate the righteousness of philosophy, then Polemarchus' formal identification of *dikaiosynē* and human excellence will prove to be robust enough to withstand the rigors of the highest intellectual peaks of the *Republic*.

20. Failure to perceive this development in Polemarchus has led many readers to complain of the inconsistency between the treatment of arts in the present passage and the Autolykean account offered only a few pages earlier. But Polemarchus has consistently viewed the arts from the perspective of justice, as he understands it. As his understanding of justice undergoes change, so does his view of the arts. There is no glaring oversight here, as some readers imagine, but only a good example of human learning.

21. If one wishes to go outside the *Republic* for confirmation, there is of course Socrates' remark in the *Phaedrus*, affirming that "Polemarchus has turned toward philosophy" (*Phaedrus* 257b3–4).

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THOUGHTLESS ASSERTION AND POLITICAL DELIBERATION

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Predominant views of political deliberation represent it as a matter either of "contemplation without interest" or of interest without contemplation. Whether we claim that political thinking can transcend power or that it is simply a vehicle for it, we abet a nihilistic political culture and authorize the thoughtless exercise of power. What each view denies—or insufficiently explores—is thinking's capacity to transfigure *"interest" and power without pretending to transcend them. By contrast, political perspectivism incorporates multiple perspectives in an effort thoughtfully to respond to the will-to-power that attends our location in the world. We need political spaces if we are to become responsive agents of power, because we often can neither recognize the effects of our activities without the benefit of others' perspectives nor alter those effects by our efforts alone.*

> Thought must consider the process that forms it and form itself from these considerations.
>
> —Foucault, *Language, Counter-memory, Practice.*

Taking the canonical texts of Western political theory as our guide, we could conclude that the activity they represent, *political thinking,* is at best paradoxical and at worst impossible. With few exceptions, thinking (usually equated with philosophy) and politics (derogatorily associated with power and opinion) have been judged antithetical. This conclusion is moored in contestable beliefs about the relationship of power and thinking, body and mind. These assumptions both impoverish thinking and consign politics to the thoughtless exercise of power. Reading Friedrich Nietzsche and Hannah Arendt together, I challenge these views and offer another perspective on political theorizing.

The *body as figure* is one of Nietzsche's primary vehicles for disparaging the views of others, as well as a means of enacting his own, alternative way of thinking.[1] Nietzsche's critique of objectivism, or "contemplation without interest" is well known (1967c, 118–19). But his critique of subjectivism or interest without contemplation, is often ignored, so that Nietzsche is still often converted into an advocate of some variant of irrationalism or ruthless assertion. To interpret Nietzsche's writings in this way is, however, seriously to misconstrue them. As Nietzsche wrote in *The Gay Science,* "the greatest danger" is "the eruption of arbitrariness in feeling, seeing, and hearing, the enjoyment of the mind's lack of discipline, the joy in human unreason" (1974, 130). In other words, the greatest danger is the repudiation of thinking and its consequences. Both "contemplation without interest" and interest without contemplation are aspects of such thoughtlessness and manifest the unitary point of view that Nietzsche associates with nihilism (1968, 70). By contrast, as a perspectivist, Nietzsche is attentive to will-to-power (evinced in the plurality of *bodies* and their effects) and its potentially *affirmative* relationship to thinking's capacities. Perspectivism is located, interested thinking and its goal is the incorporation of multiple perspectives in the service of controlling "one's Pro and Con" to refashion who one is becoming (1967c, 119). The transfiguration of the *body* proves to be a potent metaphor in pursuit of perspectivism's goal.

Locating thinking, or "reason[ing] the particular," is usually conceived as a matter of judgment, whether aesthetic, moral, or political. Hannah Arendt has furthered our understanding of this reflection attuned to particulars.[2] While there can be no doubt that Arendt chose Kant as her primary touchstone for understanding thinking and judgment (Arendt 1977, 197–264; idem 1978; idem 1982), the heavy reliance on him obscures a rich, Nietzschean vein in her political theory.[3] Contrary to the most common view, Nietzsche may further our understanding of the ways of thinking that make specifically *political* power and deliberation possible.[4] He thereby prefaces (though he himself does not pursue) a revaluation of politics.

In Arendt's view, Kant needs her for his political philosophy to appear (Arendt 1982, 7–8). Nietzsche also needs her if we are to conceive the significance of his writings for political thinking. Indeed, if perspectivism is to be actualized or to avoid repugnant forms (Fowler 1990), it must (contra Nietzsche) be *political* perspectivism, because (as Arendt teaches us) politically constituted commonality is a condition of the appearance and worldly effects of a plurality of perspectives. In short, perspectivism is necessarily a political practice.[5] But similarly, Arendt needs Nietzsche—and in ways she does not acknowledge. I wonder whether the political spaces that Arendt commends can be created, sustained, and esteemed without keeping before us what Nietzsche teaches about the relationship between thinking and power. Contrary to Arendt, I contend that political thinking must be *perspectival,* because only by so responding to our bodies, *"private" locations,* and their effects can we initiate the political spaces in which plurality appears and is, if transfigured, preserved. In short, word and deed are political only when they seek to enact perspectivism.

Both Arendt and Nietzsche challenge predominant views that equate thinking with knowing and thus with the search for truth, rather than meaning (Arendt 1978, 14–15, 57–65).[6] Arendt follows Nietzsche in disclaiming the distinction between "reality" and "appearance" (e.g., Arendt 1978, 10; Nietzsche 1954, 485–86; idem 1968, 305–6) but her characterizations of "mental faculties" sometimes verge on reinstating the distinction.[7] Nietzsche's corporeal thinking registers thinking's effects in the world. Arendt wondered whether thinking, as an activity that disturbs given or thoughtless truths, might have such effects (1971, 418; 1978, 71), but her views about the political relevance of the body unduly circumscribed her efforts to elaborate them. Just as Arendt's acute sense of the relationship between political spaces and the appearance of a plurality of perspectives enables us to see the political-theoretical significance of Nietzsche's writings, his transfiguration of the relationship between body and mind clarifies what worldly, political effects thinking might have. Together, Nietzsche and Arendt help us (re)imagine political theorizing, and the enhanced political vision that emerges from an encounter between their writings attests to the fertility of such political perspectivism as mutual transfiguration among different locations and views.

TRANSFIGURING THE BODY OF THE "THEORETICAL MAN"

An interpretation claiming an affinity between Nietzsche's writings and political theorizing may seem implausible, given Nietzsche's animosity for the "theoretical man" Socrates (e.g., 1967a, 82–86, 94–98, 109–14; 1954, 473–79). Nietzsche claims that Socrates and the dramatist he inspired, Euripides, are responsible for the degeneration of "the old Marathonian stalwart fitness of body and soul" that once distinguished Athens (1967a, 86). The corruption of tragedy marked this decline. The myopic Socrates turned his "one great Cyclops eye" on tragedy, "an eye in which the fair frenzy of artistic enthusiasm had never glowed," and shunned it (p. 89). According to Nietzsche, Socrates found tragedy neither true, nor beautiful, nor (for that matter) useful. The "artistic enthusiasm" that in Nietzsche's view, Socrates did not know and Plato later would not countenance Nietzsche honors because he believes it both reflects and best captures life: "for life is based on semblance, art, deception, points of view, and the necessity of perspectives and error" (p. 23). Already we sense the doctrine of perspectivism, which claims that there are "no facts . . . only interpretations" (1968, 267).

In later works, Nietzsche joins together the artist and theorist, as it were, claiming that criticism always entails affirming and creating: "We can destroy only as creators" (1974, 122). Nietzsche's objection to the "theoretical man" lies in the latter's denial of this creative aspect of thinking—in Nietzsche's terminology, denial of his will-to-power. Both the artist and the theorist are "critical" (1967a, 93–98), but to Nietzsche's way of thinking, the artist is more so because he understands that living necessarily entails both interpreting and affecting the world and therefore acknowledges and creatively directs his will-to-power. The theorist who emulates the artist, Nietzsche believes, is less likely to delude others or himself.

The first intonations of Nietzsche's alternative way of theorizing are found alongside this critique of Socrates when he celebrates the dramatist who possesses sufficient power to transform himself and speak out of the bodies and souls of others (1967a, 64). This foreshadows Nietzsche's affirmation of the philosopher "who has traversed many kinds of health, [and] has [thus] passed through an equal number of philosophies" (1967c, 96; 1974, 35). Nietzsche's appreciation for this dramatic talent—"to see oneself transformed before one's eyes and to begin to act as if one had actually entered into another body, another character"—prefigures perspectivism (1967a, 64; 1974, 132–33). The dramatist's talent is the raw material for new thinking habits.

Nietzsche persistently uses corporeal and sensorial metaphors, often unconventional ones, to describe and distinguish various ways of thinking.[8] This poses the question of why, when faced with the "need for a new world of symbols," he, like the Dionysian, called "the entire symbolism of the body into play" (1967a, 40). I suggest that thinking through the corporeal registers Nietzsche's alternative way of theorizing. If, as he claims, "perspectivism is only a complex form of specificity" (1968, 340), Nietzsche's use of the *body* is a potent way of attending to the locality and effects of thinking. He continually transfigures the *body*, often by telling the story of his own *body*, taking all the senses and bodily processes together, playing them against—and with—one another, to convey the constitution and transformation of viewing and thinking alike. Perspectivism is thinking incorporated and analogized by embodied, power-responsive vision.

Speaking generally, will-to-power secures the beliefs of, and practices fostering, a way of life (Nehamas 1985, 74–105, 141–69). I use the word *location* for this amalgamation of embodied habits and beliefs. Will-to-power is relational and its effects are engendered by simply living our locations, by our *body's* ways of being in the world. Consequently, location should be thought of not as a place, but as a space which is continually changing with how it is linked at any given moment to other locations. In enacting these spaces for ourselves, we are inevitably engaged (whether we are aware of it or not) in challenging other locations or in suppressing and destroying them altogether. The views and practices that prevail in a body politic reflect and reinforce patterns of congealed will-to-power. These often invisible patterns are generated by "governing powers" manifest in the norms, practices, and efficacy we bring to bear upon our own and others' conduct (Foucault 1983, 212–26). These powers are not exercised exclusively by government but are borne and extended by subject–citizens and evinced by the traces they leave on the surface of all to which their *bodies* and activities relate them (Foucault 1977, 165–96).

Even when shared ends bind us to others, differential effects, fashioned by disparate histories, come into view because we carry interpretations attesting to the particularities of our *body's* manifestations and requirements. Our pursuits resonate with, extend, resist, or are stifled by force fields across a body politic. Conflicting, unequal effects are continually manifest in subject–citizens' mutual relations and engendered by their relation to norms, the will-to-power that accrues with them, and the psychological and material consequences that flow from this positionality or location. Our capacities and efficacy are increased or decreased by our degree of conformity to the norms embodied in civil codes even though we do not possess these powers but bear them. Our efficacy is contingent upon our ability and willingness to flow with them. In doing so, we may have harmful effects upon others who are differently aligned in relation to extant patterns of will-to-power. As a result and with our aid, selves and world are made pliant to some persons and projects, recalcitrant toward others. For instance, our *body's* appearance (e.g., how it carries "race" and "gender") calls forth responses from others and also has (often unintended) effects upon other *bodies* and the world—effects that emanate from the activities and the ends that it "needs," demands, or is "empowered" to acquire. All of these are a manifestation of our unavoidable will-to-power, of which Nietzsche says, "This necessary perspectivism by virtue of which every center of force . . . construes all the rest of the world from its own viewpoint, i.e., measures, feels, forms, according to its own force" (1968, 339). When we deny our will-to-power, we ignore the effects attending the perpetuation of our *bodies* and locations in the world—in which case, politics and culture become matters of thoughtless assertion.

If our thoughts and *bodies* are so bound, it does not necessarily follow that thinking therefore can only reflect our *body* and its interests (as so many have concluded it does).[9] Thinking, especially when we attend to location (the continually changing space in which our *body* appears) can enable us to alter our "interests" and effects, though we cannot altogether transcend them. While we may be beings who are always becoming, we are nonetheless subject to patterns of will-to-power—to specific habits and related limits of perspective—that elude our thinking when we are without the aid of perspectivism and (albeit to a lesser degree) even when we avail ourselves of it. The corporeality of Nietzsche's thinking conveys the weight of the world and our *bodies* while avoiding determinism (Nietzsche 1967c, 129; see also Blondel 1991, 201–38). The genealogist understands the will-to-power that inscribes and incorporates preponderant dispositions and habits, but the genealogist also knows that there is a degree of contingency (and thus human agency) within processes of inscription and incorporation that hold out the promise of alterability (e.g., Foucault 1977, 148–64). Nietzsche thinks corporeally to convey both the possibility of transfiguration and the weight of all that is poised against it.

Philosophers, Nietzsche believes, have traditionally been predisposed to disregard the *body* or to neglect its role in thinking.[10] He identifies the root of this "peculiar philosophers' irritation at and rancor against sensuality" in the ascetic ideal (1967c, 106). Philosophy's asceticism forecast the conception of thinking that it valorized. The ideal that affirmed "voluntary deprivation, self-mortification, self-flagellation, [and] self-sacrifice" also sought to "contemplate without interest" (pp. 118–19). Nietzsche regards the effort to do away with interestedness and the *body* as an attack upon thinking, because to conceive of a thinker who contemplates without interest demands thinking of a disembodied eye "turned in no particular direction, in which the active, interpreting forces, through alone which seeing becomes seeing *something*, are supposed to be lacking" (1967c, 105, 118–19). The objectivist's denial of thinking's creative aspect and interested character ends in the atrophy of thinking, when the "objective man" (the "philosopher," as well as his heir, the "scientist") takes "mirroring" as his ideal and submits to "whatever wants to be known" (1973, 115). Mirroring, the suspension of "interestedness," is actually a refusal that eventually becomes an inability to think. Contemplation without interest, like "all great things [that] bring about their own destruction through an act of self-overcoming," ends in interest without contemplation and, at bottom, in meaninglessness, nothingness (1967c, 157, 161). As we shall see, to think without interest or will-to-power is impossible, in Nietzsche's view, because to think without interest is ultimately to be thoughtless (pp. 103–6; 1973, 131).

Perspectivism represents Nietzsche's revaluation of the relationship between mind and body, thinking and power; it is "interestedness" transformed through thinking. The perspectivist, by bringing other views to bear upon habits and beliefs, heightens attention to the limitations engendered by a particular location. What is unthinkingly accepted or enacted may become unsettled by incorporating alternative views. The relationship between thinking and power is also transfigured, for perspectival thinking is not subject before will-to-power or merely a vehicle for expressing "interest." Rather, will-to-power is potentially transformed through thinking, and the latter, in turn, may be ennobled by the power of others' perspectives. Nietzsche reconceives "objectivity" in his penultimate expression of perspectivism: "there is *only* a perspective seeing, *only* a perspective 'knowing'; and the *more* affects we allow to speak about on thing, the *more* eyes, different eyes, we can use to observe one thing, the more complete will our 'concept' of this thing, our 'objectivity' be" (1967c, 119). The "definition" or "essence" of anything, Nietzsche maintains, is finally an opinion about a thing and an effect of interpretation (1967c, 79–81; 1968, 301–2). Nietzsche's refiguring of "objectivity" resists the epistemological commitments of discourses structured in accordance with a *monologic* truth. Instead, he favors thinking as the incorporation of multiple and diverse views in pursuit of fuller opinion, the proliferation of meanings, and the sustenance of a plurality of perspectives. Will-to-power thereby becomes less reactive and impositional, more thought*ful*.

When Nietzsche narrates the expansion and change of his bodily processes and senses, he is figuratively conveying his effort to alter his views by thinking or incorporating other perspectives. Bringing "many eyes" to bear on any one thing requires "the ability *to control* one's Pro and Con and to dispose of them, so that one knows how to employ a variety of perspectives and affective interpretations" (1967c, 119). Nietzsche's discussion of *brief habits* begins to suggest (we will need Arendt's perspective to understand more fully) how we may come to critically evaluate and transform our locations and their effects, if never altogether surpass them. Nietzsche loves brief habits because he considers them "an inestimable means for getting to know *many* things and states, down to the bottom of their sweetness and bitternesses" (1974, 236–37). To appreciate something, one must experience it with a passionate faith, with the belief that one has found the thing that will provide lasting satisfaction. This is so because "untruth is a condition of life" (1967a, 23; 1973, 17). It is not merely a matter of not being *able* to subject all of our beliefs and habits to criticism all at once. Even if one could, to do so would be prohibitive. Immersion in the world and (at least temporary) partiality make seeing and thinking possible. Affirming one's historicity is a condition of new thinking and action (e.g., Warren 1988, 192–94).

Either a life of enduring habits or one without any habits at all, according to Nietzsche, would prove inhospitable to thinking. On the one hand, a life of enduring habits is monolithic and without variation; the new, the original, and the many (and thus a plurality of perspectives) are its definitional antitheses (1954, 639). One may become identified with one's location until its perspective is all that one knows; and then, what is habitual is regarded as unalterable. On the other hand, a life without habits precludes the digestion and incorporation of experiences (1983, 62). Regarding one's life as a performance of "perpetual improvisation[s]" ceaselessly seeking to evade the habitual is predicated on an inability or unwillingness to discern the effects that accumulate with one's location in the world (1974, 237). Moreover, to resist or deny one's habits and the "materiality" of one's location is to overlook how will-to-power and its effects not only limit vision but may also enhance it. Perspectivism requires one neither to live one's *body* and location thoughtlessly nor to believe that they are so plastic as to be transformed at will. Rather, it calls for altering "who" we are or how we bear the effects that accrue with "what" we are (or our embodied location in the world) first by striving to recognize their potential brevity.[11]

Learning to love and desire something is to turn it into a habit. Like Socrates, who found eros and thinking inextricably bound, Nietzsche maintains (though with different effects) that "all great problems demand *great love*," love being something that one must learn by first (often with difficulty) detecting and distinguishing something and then *"tolerat[ing]* it in spite of its strangeness" until it finally becomes a part of who one is: by offering hospitality to what initially appears strange, we learn to love and to think (1974, 262). Both require responsiveness to "strangeness"—a willingness to acquire new habits or (at the very least) to have one's own habits questioned. Brief habits are kept from becoming thoughtless truths when we incorporate views that open up the possibility of transfiguring our ways of life.

The love that one feels for one's problems is indicative of a personal relationship to one's thoughts (Nietzsche 1974, 283). Personal thoughts threaten to become cold and impersonal when they take on the appearance of unassailable truths and their temporal specificity and emanation from particular locations and lives is no longer palpable (1973, 201). Such truths simultaneously attest to the power and impotence of thinking; to the destructive yet compelling character of its deracinated forms. Boundless thoughts and the beliefs and habits that appear to command universal assent or consensus are remarkably potent: they rule minds. The individual mind, in its weakness, becomes subject before such thoughts and, forsaking its own, loses a plurality of perspectives. Thinking is at risk if the particulars or habitual character of a thought can no longer be fathomed. There is danger in brief habits becoming "immortal truths" (Nietzsche 1973, 201; see Arendt 1971, 435; idem 1978, 176). When the latter congeal, we approach nihilism.

OUR NIHILISTIC POLITICAL CULTURE

Thoughtlessness is a primary aspect of what Nietzsche calls nihilism. Nihilism exists when there is a predominant sense either that everything is permitted because there is no basis for choosing between things or that all such bases are arbitrary, in turn possibly leading either to resignation and the feeling that all is meaningless or to an aggressive indifference. One of nihilism's chief effects is a crisis in human action (Warren 1988, 13–45), a dissipation of thoughtful efficacy. We are thoughtless when we do not or cannot conceive or recur to the effects attending the perpetuation of our locations in the world. The consequences of doing so are enormous. When the contours of a particular life and location are not perceived because the bounds of one's views are not within the purview of thought (Nietzsche 1973, 114–16), then what is actually bound and limited will appear boundless and rule one's mind. Indeed, if one's way of life is a prevalent one, these views receive confirmation everywhere one looks and will appear commonsensical. A unitary perspective prevails by virtue of our incapacity to rejoin what is strange, challenging, or invisible to us.

Another consequence of such thoughtlessness is a return of the repressed. Will-to-power, once denied, now may appear everywhere and seems to overcome thinking. This situation is often precipitated by those whose views are dismissed or elided when prevailing yet limited views are represented as boundless; "genealogical" cultural criticism documents the will-to-power evident when particular perspectives on the world mask themselves as universal or commonsensical. Such critiques are often regarded as an assault upon the possibility of "objectivity" however conceived. This is the state of affairs characteristically named "nihilist" (and "Nietzschean") in the United States (e.g., Bloom 1987, 141–240).

Thus, the contemporary intellectual, cultural landscape appears to be divided among distinct camps.[12] On one side, are "traditional" philosophers and scientists, who maintain that one who thinks can surpass *interest,* that is, break the ties that bind thinking to power. (The latter is sometimes equated with domination and coercion, more often with the "subjective" and "irrational.") Indeed, to transcend will-to-power and to dissipate or dispel interest are thought to be among the chief characteristics and virtues of thinking (which, in this case, is usually equated with knowledge). The political outlook corresponding to the perspective of this first camp holds that faction and differential effects can be exceeded and neutral or reasonable ground (whether scientific or moral), found or created.[13] Critics of the first camp maintain that thinking is inextricably bound to, and serves, will-to-power (albeit in sophisticated and convert ways). So far, this second camp has tended (though need not necessarily) to view politics as a battle of competing interests with outcomes determined by their relative strength.[14] Perhaps more often, a reconciliation of the first camp's "contemplation without interest" and the second camp's interest without contemplation is sought in some notion of "enlightened self-interest" when expert knowledge or mature, "postconventional" reasoning provides the thoughtless ego with its "real" interests, again with the danger of shielding thoughtless assertion behind claims to "knowledge" or "rationality."[15]

From the perspective afforded by (if not always propounded in) Nietzsche's writings these philosophical and political positions may be judged dogmatic, if in different ways. *Together,* those who claim that thinking can transcend will-to-power and those who claim that it is simply a vehicle for it (if anyone truly holds the latter view) abet our nihilistic political culture. What each denies (or insufficiently explores) is the capacity of thinking to transform will-to-power without pretending to surpass it. By contrast, Nietzsche denies neither thinking's proximity to will-to-power nor the capacities of thought. He affirms the possibilities of thinking in a world saturated with will-to-power's effects. On this understanding, Nietzsche's perspectivism provides us with a way of conceiving the interrelationship of thinking and power that does not allow the ascension of either term but facilitates the fullest understanding of their unavoidable, and potentially affirmative, interdependence. Political theorizing attends and responds to will-to-power. Perspectival thinking entails recognition of both how one becomes "what" one appears to be and to what views one's location gives inclination, combined with a facility for incorporating alternative perspectives and thus for transfiguring "who" one is becoming.

PLURALITY, COMMONALITY, AND POLITICAL SPACES

Nietzsche is not a political theorist. His interpretation of nihilism indicates its cultural and collective sources but without due or satisfying attention to its political aspects or preconditions. In my view, he disavows that nihilism can be collaboratively alleviated. He surveys modern culture from his particular location within it and determines that few, if any, practice the thoughtful self-transfiguration he esteems. He may be right, though he does not tell us why most appear to be incapable of this thoughtfulness, enhanced vision, honesty, and courage before themselves and the world.[16]

Hannah Arendt regarded modern thoughtlessness from a different perspective. Though this principal concern of her work is most conspicuous after *Eichmann in Jerusalem* (see Arendt 1964, 1965, 1971, 1978), her intuition about connections between reflective capacities and political possibilities is evident much earlier (Arendt 1951, 1953, 1958). On my understanding, Arendt's insight is that thinking (or, negatively, thoughtlessness) is conditioned by the configuration (or absence) of political spaces and by the availability and quality of specifically political power. While both Nietzsche and Arendt value and presuppose plurality, they have quite different views of its relationship to political commonality. For Nietzsche, the latter threatens plurality and thus his (anti)political opinions. By contrast, Arendt believes that the appearance of plurality and its worldly effects become manifest through politically constituted commonality. For Arendt, the ascendance of a unitary perspective (which she, like Nietzsche, associates with nihilism [Arendt 1971, 435]) and the destruction or imperceptibility of a plurality of views are linked to the absence of distinctly political spaces and the political commonality they potentially promise (1958, 58). The appearance of the plurality that Nietzsche values is, in Arendt's view, impossible without political spaces. Yet Arendt often imagines these spaces (I think mistakenly) as necessarily surpassing the demands of the *body* or "what" we are. We need both of their perspectives, altered by mutual transfiguration, to conceive political perspectivism. The challenge for political thinking is to suggest how commonality may be politically constituted while recognizing that all such political instantiations themselves are manifestations of will-to-power's effect. Additionally, we need stories telling how these effects may be politically addressed without devolving into thoughtless assertion, that is, without either denying that such effects repose in thinking or simply exercising them thoughtlessly.

According to Arendt, contact between human beings facilitates the appearance of both commonality and plurality. Where people speak and act together politically, such spaces engender and sustain whatever perception of commonality we achieve (Arendt 1958, 50, 52–53). This in turn facilitates recognition of our distinctness from other persons and the revelation of individual uniqueness or "natality" (pp. 49, 175–76, 178). Paradoxically, the perception of commonality creates boundaries, or at least a capacity to recognize boundaries as well as connections between oneself and others (p. 52). If political commonality is absent or unimaginable, then not only what we potentially share with others but also what distinguishes us from them (our distinct perspectives upon and locations within the world) become precarious (pp. 57–58, 199). Whatever assails the possibility of political commonality menaces plurality and vice versa. This is Arendt's view of our greatest threat; for "mass society not only destroys the public realm but the private as well, deprives men not only of their place in the world but of their private home" (p. 59). Perception of commonality in the world needs the presence of a multiplicity of perspectives that despite their diversity create and confirm its sharedness (pp. 57–58). "The end of the common world has come when it is seen only under one aspect and is permitted to present itself in only one perspective" (pp. 39, 58, 220). While political spaces create commonality, we each have our different locations in the world and our views are a mixture of "who" we are as revealed in such spaces and the distinct locations that condition "what" we appear to be. Different locations afford differentiating perspectives on a common thing (p. 57). Political commonality enables this plurality to appear.

Arendt is notorious for advocating a hard-and-fast boundary between matters *public* and *private* (Arendt 1958, 22–73, 192–220) in the interest of preserving the distinctness of political commonality from "biological sameness." Through political action, we leave behind (or silence for a glorious moment) the necessities of "biological life process" and "what" we are in exchange for freedom in the public world. When she speaks in such terms (and she does not always), Arendt's political theory has a rarefied quality that obscures its relevance to modern liberal democratic polities in which people seem preoccupied with their *bodies* and interests. But the boundary Arendt draws between public and private matters is eroded by her own insistence on the intrinsic import of plurality for the actuation of political commonality. Plurality is indispensable to the emergence of specifically political spaces and the contour and character of our locations begin plurality's content. Though contrary to some of her most explicit views, Arendt's emphasis on plurality suggests that one's private location and *body* ("what" one is) weigh upon "who" one is revealed to be in political spaces.

Arendt neglected the interrelationship between *bodies*, locations, and specifically political power, because, unlike Nietzsche, she conceives the *body* as politically irrelevant. This is in part because Arendt treats the *body* and its "necessities" as univocal and unalterable (e.g., Arendt 1958, 46, 111–12, 119, 123; 1978, 22, 29, 33, 35, 44), rather than—as Nietzsche maintains—contingent (if weighty), culturally and politically inscribed (sometimes even arbitrarily), and thus contestable and politically significant. Nietzsche recognized that the *body* as we know it (and as Arendt assumes) appears as an undifferentiated mass, but he also maintains that "the welding of a hitherto unchecked populace into a firm form [like Arendt's "mass society"] was not only instituted by an act of violence but also carried to conclusion by nothing but acts of violence" (1967c, 86). As we are incorporated into such a "firm form," our perspectives are, at least in part, attributable to our different *bodies* and locations, our habits and beliefs, and their relative effects.[17]

Discerning "what" we appear to be—our *bodies* and private locations—is therefore not irrelevant to, but a prerequisite (if not sufficient) for political deliberation and action. Histories of our locations may offer a critical purchase on Arendt's undifferentiated "mass society" and thereby facilitate the appearance of a plurality of perspectives. Without thinking about and being responsive to such particularities, we lack the basis for a differentiating perspective, one among the many that Arendt says are required to create and assure political commonality (1958, 58–65). Likewise, when political spaces are absent, the distinct perspectives that one's location(s) allow become indiscernible and, again, both plurality and commonality are at risk, and with them political agency and the capacity thoughtfully to affect the conditions of one's life. Absent political commonality and the plurality of perspectives accompanying our different locations, we may lose our world, figuratively and literally (1951, 135–47, 269–302, 460–79; 1958, 57–61, 208–9). Perhaps Arendt discounted the political relevance of "what" we appear to be because she, like Nietzsche, held such a sobering view of how human beings secure their ways of life.

TRESPASSES AND POLITICAL ACTION: HOW ONE BECOMES WHAT AND WHO ONE IS

How do we forge the locations which begin plurality's content? Our locations are *made* as we master necessity by means of force, coercion, and even violence, toward nature and other human beings (Arendt 1958, 31, 129–30, 140). In Arendt's view, necessity, and therefore compulsion, persists (though in modernity it is increasingly hidden) wherever human life is sustained (pp. 121, 125, 130, 135). The satisfaction of human, *bodily* needs—needs for food, shelter, and leisure (among other things)—includes compelling nature, oneself, and others to bear the burdens of life. For the most part, in mastering necessity and fabricating locations, "making" is the relevant mode of activity and it is an indispensable manifestation of the *vita activa* (p. 136–74).[18] But making, as principally a matter of *poiēsis*, consists in the mastery of others and oneself; thus, an element of violation—and potential violence—inheres in all fabrication (pp. 139, 194–96). By itself, making creates a world where necessity and compulsion, not freedom and political action, predominate and with destructive repercussions for human relationships (pp. 195, 220–30, 233–36).

The means by which each location is made—and, in turn, is affected by others' locations—affords each some of its specific character and commences its differentiating perspective on the world. While Arendt's reflections on the "private realm" tend to represent location as a singular place or "home" her notion of *trespasses* belies this narrow understanding (1958, 28–37, 58–67, 236–243). Through our activities, we insert ourselves into a weblike world. Through our words and deeds, whether in political space or some other, we make a world, disrupt what precedes us, and may initiate something altogether new (pp. 8–9, 188). When we forge our locations, we *trespass* against others (p. 240). Her reflections on *trespasses* show Arendt, like Nietzsche, to be a theorist of will-to-power.

Trespasses are not "radical evil" but signify its everyday variety and they adhere in the very nature of the *vita activa*'s establishment of new relationships (see Arendt 1958, 240–41; idem 1971, 438). *Trespasses* are effects that one cannot undo oneself, and they occur under circumstances where one did not, or could not, have known what one was doing. One may not have known what one was doing because the effects of human activities cannot be predicted (1958, 188–92). It is the nature of these activities to exceed our intentions and plan because we share a world we did not make with other human beings who are also makers and actors (1953, 389; 1958, 184, 190). Moreover, we are not responsible for "what" we are—how and what we have been "made" to be by history, institutions, and patterns of power that preceded us and for which no one person alone is responsible (e.g., Nietzsche 1954, 500–501). We may become responsible, however, for "who" we are, that is, for how we bear the will-to-power that accompanies "what" we appear to be.

It is unlikely, however, that we can become responsive to our will-to-power without political spaces, for when we trespass we also may not know what we are doing because we often cannot recognize the effects of our *bodies* and locations without the benefit of others' perspectives. One must learn about these effects from others, usually in the form of a story told about or from the perspective of those others whom one effects. Until then, one's effects and actions (though they are apparently one's own), may remain largely unseen or unacknowledged. "Who" we are only appears to us in the form of an unending story told by others (Arendt 1958, 181–88; Nehamas 1985, 163–69) and is distinguished from "what" we appear to be by how we bear and transfigure the effects of our will-to-power. But we can do so only in political spaces where we act in concert with others because our *trespasses* are extended by patterns of power that we alone cannot change.

The distinction between "necessity" and "freedom," "making" and "political action"—so crucial to Arendt's political understanding—turns on the degree of articulation in the latter term of each pair. Freedom and political action define spaces where we "answer," "talk back," and "measure up to" what has happened or what we have done, the effects of our location in the world (Arendt 1958, 26; idem 1982, 41). Necessity and making define relations that are leveling, forceful, and unresponsive. When necessity and making reign, as they typically do in the daily (nonpolitical) fabricating of our locations, we simply have effects upon others and receive their effects upon us; neither we nor they are called to answer for them. Will-to-power is exercised thoughtlessly. Combative, not collaborative, action prevails.

By contrast, political spaces both may provide ways of reckoning the necessity that reigns outside and may be carried into political spaces.[19] In Arendt's view, the "ruling and being ruled" typical of making (and which likewise inform modern understandings of politics to the extent that we confuse it with "governance" and its powers) are "prepolitical," because they are relations of necessity, rather than freedom—of making, rather than political action (Arendt 1958, 220–30). The public or private character of relations is less decisive for their being political or nonpolitical than is whether we fathom and respond to our effects upon one another and open them to specifically political deliberation and action. "Who" we are may be judged estimable when we become responsive in this way.

On this understanding, politics is not so much a realm or sphere we enter into as it is a form of relation we undertake. Political spaces do not simply reveal or provide a means of furthering our will-to-power. Rather, in them we encounter others who call how we bear its effects to our attention, and we may do likewise. We constitute political relations and create the possibility of political commonality when we think together about the ruling and necessity—compulsion and violation—that dwell in all "making," whether they become manifest in individual acts, social behavior, or governance. Indeed, if we do not, spaces of cooperative action are undermined then destroyed by thoughtless assertion. In political spaces, we have the opportunity to deliberate—to discern and answer for our effects and, at least potentially, to transfigure them by means of the political power generated when we act in concert. When we associate politically, we enact perspectivism and thoughtfully direct our will-to-power.

TRANSFIGURING THE BODY OF THE POLITICAL ACTOR

It remains to be seen, however, exactly how political deliberation reveals and, most importantly, transfigures our distinct perspectives. How does perspectival political thinking help us to come to see the *trespasses* (unknown, unintended, thoughtless effects) that "made" our locations and the collective histories of which they form a part? How does it facilitate our acting in concert so that we can generate the collaborative power needed to transfigure our assertive effects and the patterns of solidified will-to-power which embolden them? In my view, Arendt's most suggestive formulation follows, in which she conceives political thinking as "representative":

> I form an opinion by considering a given issue from different viewpoints. . . . This process of representation does not blindly adopt the actual views of those who stand somewhere else, and hence look upon the world from a different perspective; this is a question neither of empathy, as though I tried to be or feel like somebody else, nor of counting noses and joining a majority but of being and thinking in my own identity where actually I am not. (1977, 241)

Political thinking imagines other views. But what is Arendt practically endeavoring to describe? The Kantian notion of "enlarged mentality" on which Arendt draws in this and related passages (e.g., 1977, 197–264; 1982, 44) has become a popular locus for efforts to conceive political judgment. The virtue of Arendt's rendering of this idea, at least when we read it with eyes transfigured by Nietzsche, is that she does justice to its difficulties. Too often, the imperative and possibility of regarding matters from another's point of view or of "reversing perspectives" are simply presupposed. Though Arendt, following Kant, often invokes forgetting the *body* and "what" we are as the goal of such reflective activity (e.g., 1978, 30–45, 1982, 54), Nietzsche's refiguring of the relationship between body and mind—power and thinking—counsels against it. The goal of perspectival political thinking is not to exceed will-to-power but to transfigure it into the collaborative power we need if we are thoughtfully to (re)direct our effects. Read in light of the shadows cast by Nietzsche, Arendt's formulation suggests just how difficult matters can be when we endeavor to grasp another's perspective so that we can act together. She helps illuminate such political thinking by first telling us what it is not.[20]

First, representative thinking is not empathy ("as though I tried to be or feel like somebody else"), for it shares the vices of enlightened self-interest (Arendt 1965, 73–94; idem 1982, 43). Like "contemplation without interest" it is founded on the myth that one can consciously suspend "what" and "who" one is and adopt a viewpoint independent of interpretation. The fallacy that one can simply adopt another view is also thoughtless, like interest without contemplation, because another perspective is gained without thinking. In the course of doing so, an act of interpretation has been performed, but the denial of this fact almost assures the interpretation's arbitrary content. When one presumes to adopt another perspective without reflection on the boundaries of one's own *body* and location, more often than not one simply imposes the view from there upon another. Indeed, this is a principal way of bolstering one's location and demonstrates the effects involved in doing so. In such cases, one's own view arrogates another's and thus threatens to violate or do away with it altogether.

Neither is representative thinking, according to Arendt, a tally of viewpoints whose calculation results in a majority "perspective." This is the case for two reasons. First, it it so because, like interest without contemplation, amassing multiple perspectives into a single view is an abdication of thought and evaluation. Here the value of a view depends upon its strength or numbers, and politics as a battle of competing interests is thoughtless. Second, and relatedly, this means of creating one perspective from many does not represent a full view (an aspiration toward as comprehensive a view as possible) but the imposition, again in an arbitrary manner, of a univocal view. Political commonality is confused with the ascendance of a monologic truth or consensual perspective. Such thinking is neither "representative" nor political because the viewing of commonality in only one aspect (as we saw earlier) ultimately threatens to obliterate the world in which we appear and act politically. According to Arendt, the creation of commonality depends upon the preservation of a plurality of views. A representation of what is common that forgoes this plurality of perspectives and obscures or destroys each differentiating relationship to it threatens to render both our locations and world indiscernible.

The key to Arendt's alternative and, by implication (I am contending), an elaboration of the political theoretical significance of Nietzsche's perspectivism, is the idea of "being and thinking in my own identity where actually I am not." How does one, like Nietzsche's dramatist, enter into other *bodies*, other characters? Arendt may be read as trying to convey a precarious balance—the preservation of one's own perspectives and political thinking capacity while incorporating another's view. Political deliberation and action should not result in the obliteration of the perspective—or an assault upon the location—of any participant. When they do, neither thinking nor deed are political but bear a greater resemblance to "making." The mastery of another's perspective is antithetical to the "answering" and "measuring up to" the perspective of another definitive of political relations. Political thinking may require the alteration of both one's own perspective and that of another but the total loss of neither.

The effort to think from another's perspective may change "who" I am—my location and how I live it. Our location is the sum of our effects and, by implication, the sum of all that effects us (Nehamas 1985, 74–105). It consists of our habits and beliefs and the sway of the world and its other inhabitants upon how we live them. To think another perspective—to endeavor to conceive another location—even if ultimately to refuse it (and no doubt we often will), entails some acceptance of that perspective. When comprehending another view, one incorporates it into what one can imagine and seeks to envision one's location from another's perspective until, finally, it may transfigure what that location is. Equally, in this responding to the view of another, the location and perspective one encounters may also be commuted. One's own perspective and another's mix to form an original combination, a new perspective on the world and an alteration of the plurality of locations that sustain both it and whatever political commonality we achieve. If one has actually come to conceive another view and to answer to it, one's habits and beliefs—one's own *body* and location—may never again be lived in quite the same way.[21] To think from the vantage of another, to respond to the perspective born of their location, and to measure up to the relation of your location to it may call for changing "who" you are. Political deliberation offers the world a modified view of another location mixed with "who" you are, thus transfiguring "whom" you and they appear to be, as well as "whom" you and they are becoming.

When we think and act politically (and admittedly, on this understanding, it is rare indeed), we come to recognize the distinctness of our *bodies* and locations and the perspectives they afford in contrast to the perspectives of others. The manifestation of such a plurality of views is not a "thought experiment" (Benhabib 1992, 158–70) but requires actual encounters in political spaces among persons and their differentiating perspectives. Most importantly, these perspectives, which become manifest through political deliberation and transformed by political power, are born of differential effects of compulsion, force, and violence, which both emerge from and shape our *bodies* and locations. Each of us carries such effects, whether or not they become manifest in a political or nonpolitical space. But a space becomes political when one's perspective ceases to be thoughtless assertion and becomes a location with effects upon others for which one answers and to which one thoughtfully responds. Political thinking enacts perspectivism. We think theoretically when we attend, and respond to will-to-power. Therefore, thinking is not antithetical to political action but is its precondition. One need not be a philosopher to think; each of us may become capable, with the aid of others' perspectives, of discerning our effects—responding to and transfiguring them. We think and act politically not only (or perhaps not even primarily) when we give strong arguments to justify or "woo acceptance" of our locations and perspectives (Arendt 1982, 72) but when we open ourselves to others' perspectives with a willingness to transfigure "who" we are becoming.[22] Such responsiveness is a condition of the action in concert that generates the collaborative power needed to transfigure our effects and the patterns of power that amplify them.

We cannot reach this understanding of political perspectivism by reading either Nietzsche or Arendt alone. Rather, their views must be brought to bear upon one another to facilitate a mutual transfiguration and the appearance of a new combination, a different perspective. Efforts to preserve Nietzsche's philosophical achievement that neglect his perspectivism or render it as entailing no necessary political preconditions are likely to fail, in my view, because they finally offer no alternative to a vision of politics and culture as a battle of competing interests with outcomes determined by their relative strength and tend to ignore or conceal the assertive effects that remain even when we endeavor to enact perspectivism. If perspectivism is to be actualized or to avoid repugnant forms, it needs to be joined to political theorizing that surmises how we may come to recognize and thoughtfully transfigure our perspectives and assertive effects in response to others and (as I will suggest in a moment) why we might become willing to do so. Arendt's writings are one potent resource for such an understanding, but they too need transfiguration. Arendt (1958) maintained that we could lose our "common world," figuratively and literally, and indeed she presciently regarded this to be increasingly our condition. But her views about the relation of mind and body—thinking and power—prevented her from fully elaborating the ways of thinking that might facilitate political spaces and collaborative power. The imperative to surpass "what" we are can amount to a willful neglect of the effects that, if unattended to, may destroy the world and our capacity to act in concert. Only *perspectival* political thinking, which discerns our *bodies* and locations and their effects, and which is the precondition of political action responsive to them, can begin spaces where plurality appears and political commonality may be constituted.

CONCLUSION

To what extent commonality and plurality (or diversity) can and should be balanced (or each sustained) remains among the most pressing and virulently debated questions in contemporary American political, social, and cultural life. As posed, the issue is a false one. Both plurality and commonality presuppose and need one another. Politically constituted commonality affords the only hope of preventing the destruction of diverse perspectives. Plurality can only be preserved through political spaces where we discern, answer for, and potentially alter the effects of will-to-power which threaten to suppress or destroy particular locations and the perspectives they engender. Absent specifically political deliberation and power, plurality is overcome in a contest of strength by a unitary perspective or by the absence of any perspective at all. Likewise, commonality needs plurality if it is not to devolve into thoughtless assertion. When not sustained by such differentiating perspectives, commonality becomes narrower and more exclusive, that is, increasingly less than common and therefore rejected or assaulted by those whose views have not given shape to it. Only when commonality is created through political deliberation and action that preserve each differentiating relationship to it can what is regarded as common actually be shared—and even then only partially and provisionally.

Political spaces can be created and maintained, however, only if we seek to restrain and redirect our will-to-power. The fact that genuinely political spaces are so rare and usually so short-lived tells us something about our ways of being in the world (see Arendt 1965, 215–81). Will-to-power is unavoidable and intrinsic to human living. The decisive issue is whether or not we direct it thoughtfully. Thoughtless assertion destroys political spaces and, with them, both commonality and plurality. It is tempting, I suppose—given the entrenched view that will-to-power poses a threat to thinking and, relatedly, that legitimacy is secured by surpassing contestability—to conclude that the only way to create commonality is by deprecating our different *bodies* and their interests in order to affirm what we are believed to share, whether that be a minimalist aversion to the outright hostilities that unbridled difference is feared to portend or more substantive communal or rational ideals. But that strategy is bound to fail because, most often, it is our different locations that make demands upon others *bodies* and the world—demands that threaten to destroy those locations and perspectives and thereby obstruct the constitution of what is shared. It is not plurality's very existence that precludes our acting and deliberating together to politically constitute commonality. Such efforts are undermined by our failure to respond to the claims about will to power which different perspectives pose.

Without encountering and responding to others' challenging perspectives I may not even recognize how "what" and "who" I am erodes political possibility and, in turn, endangers a plurality of perspectives of which mine is one among many. Much stands in the way of discerning those effects and becoming willing to answer for and transfigure them—for example, incommensurability or the incommunicability of certain forms of harm, intractable hostility and distrust, want of courage and fear of relinquishing security. But if and when we do recognize and become willing to transfigure the effects of our will-to-power, they cannot be altered by our efforts alone. *Trespasses* come with simply living our *bodies* and locations. They are an effect of activities that preceded and constituted us—of histories, institutions, and patterns of power we alone did not make—though our words and deeds reinforce and extend trespassess. We must act with others if we are to alter them, and even then our success, no doubt, will only be partial. Every political instantiation or constitution of commonality has effects such that, if commonality is not to deteriorate into thoughtless assertion, it requires a continual willingness to deliberate, to listen and respond to others' claims about our effects, and (possibly) to modify them. We will always need politics, not because everything is finally determined by a contest of strength but precisely to constrain and redirect thoughtless assertion into the thoughtful exercise of will to power.

From where do we acquire the energy and inclination to become political perspectivists? Why would we take this leap of faith? When we understand that if we do not, plurality is threatened and with it the meaning and existence of our agency, distinct location(s), and world. The will-to-power embodied in our words and deeds may so harm others as to threaten to obliterate the differentiating view they have—one among the plurality that the constitution of political commonality requires. The existence of political spaces that foster the distinct views engendered by my location in the world—and thus the maintenance of that location—calls for the perspectives of "others." Without their views, the uniqueness of my own views will not appear. Unable to discern the particularities of my own *body* and location, I will be unable to constitute political spaces with others where my perspectives—and the location from which they emerge—can be revealed and sustained, if transfigured.

Notes

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1. The meaning of *body* shifts throughout my text, because Nietzsche and Arendt conceive its relationship to politics differently—and neither to my satisfaction. I shall introduce the notion of *location* to refer to the continually changing space in which our *body* appears and thus seek to keep in play the *body's* nonconscious and material, individual, social, and political significances.

2. Kant's *Critique of Judgment* was Arendt's principal resource in doing so (Arendt 1977, 197–264; 1978; 1982). Numerous commentators have adeptly evaluated the perspicacity and limits of Arendt's view of judgment and of Kant's own, especially in relation to Ariesotelian *phronesis* (e.g., Beiner 1982, 1983; Benhabib 1992, 121–44; Bernstein 1986, 221–37; Castoriadis 1991, 81–123).

3. Those who bring out the Nietzschean influence often return in the end, with Arendt, to Kant (e.g., Villa 1992) or see that influence as leading away from political action (e.g., Beiner 1982, 153–54). Honig interprets Nietzsche and Arendt as together furthering our understanding of political action 1993, 40–125; but if I read her correctly, she retains something of Arendt's antithesis between "thinking," or "theorizing," and "acting" that I query here (though see pp. 59–60).

4. Exceptions to the common view, though to varying degrees, are Connolly 1987, 1991, 1993; Honig 1993; Kateb 1992, 127–51; Warren 1988. Throughout, I use *will-to-power* to refer to the effects engendered by human activities. While it is crucial to appreciate distinct and various instantiations of will-to-power (e.g., violence, strength, force, and political power or action in concert [Arendt 1972, 143–46]) these forms of efficacy often slide into one another (e.g., as political power recedes, violence or force often emerge to take its place, pp.

105–84)—hence the need for a general term to represent this continuum.

5. Typically, we take a writer's express views about politics as indicative of his or her political significance. Nietzsche's writings have been interpreted as illiberal or protofascist (e.g., Schutte 1984 and Strong 1988); as without any contemporary political significance whatever (e.g., Nehamas 1985); as advancing an exemplary "agonism" (e.g., Connolly 1991 and Honig 1993). Some truth resides in each interpretation. Yet a writer may proffer a theoretical contribution that exceeds or even contradicts such explicit views. Nietzsche's rejection of moral valuation, which confounds the "cause" of a thing's "origin" and it utility (1967c, 76–81) in effect recommends that we not take his political views (the "origin" of his writings as it were) as determinative of their theoretical "utility." All concepts and organisms are a whole synthesis of meanings, and "only that which has no history is definable" (p. 80). Nietzsche has his own views about the value of certain concepts or organisms and judgments about the worthiness of political, collaborative power. He would be the first to admit that these are his own, partial interpretations (1973, 34) and to my mind, these views may be distinguished from perspectivism itself, which embodies his *way* of thinking. Nietzsche does not respond to a plethora of meanings and their indeterminacy by asserting a single, utilitarian definition; for he regards such monologism as dogmatic (Nehamas 1985, 33–41, 56–57) and nihilistic but attempts (though he does not always succeed) to view the question of something's value from multiple, diverse perspectives. "Value for what?" and "Value from what perspective?" are his leading questions (1967c, 17, 20, 55). They exemplify Nietzsche's perspectivism in its simplest form. Nietzsche is not always the best perspectivist (in part because he neglected, I contend, perspectivism's political preconditions), but that in itself need not discredit perspectivism as a way of conceiving thinking. Bracketing our initial impressions of Nietzsche's political views may be the best way to assess the political theoretical significance of his writings. Engagement with those writings affords a different perspective on what it means to think theoretically.

6. I must ask the reader to resist the understandable but nonetheless unjustified assumption that political theorizing must necessarily be epistemologically grounded in the stricter senses of the term. It is more misleading than helpful to conceive perspectivism as an epistemology, at least as the latter is conventionally construed (Nehamas 1985, 73; Warren 1988, 94). Moreover, Nietzsche and those who have borne his influence reject the dual-world metaphysics upon which a distinction between judging and thinking rests (Foucault 1977, 177–78). Similarly, I speak of "thinking" rather than "judging" because I regard "imagining invisibles" (in addition to perceiving what is "close at hand" or commonsensical) to be both possible while acting politically and a condition of doing so.

7. They verge on this, for example, when Arendt speaks of thinking as necessarily solitary, a withdrawal from the world, and a forgetting of the body (e.g., 1971; 1978, 22–23, 35, 44–45, 64, 69–80, 85, 92; 1982, 73). While she sometimes qualifies or contradicts these characterizations, her view of the political relevance of the *body* and its largely negative influence on "mental faculties" remains relatively constant.

8. On Nietzsche's use of the body, see Blondel 1991 and also Deleuze 1983, 39–72. For specific examples of Nietzsche's unconventional sensorial and corporeal metaphors, see 1967b, 222–23, 236–40; 1967c, 23; 1973, 55, 126, 134–35.

9. Thomas Hobbes is an exemplary theorist of this view.

10. We would have to include among them Arendt 1958, 1971, 1978, 1982.

11. Arendt, following Augustine's *Confessions* 10, introduces a distinction between "what" and "who" one is (1958, 10–11) that I shall interpretively elaborate. "What" we are represents what *appears* to be given or beyond the reach of our agency, while "who" we are is revealed through our deeds. Since "what" we are cannot finally be known, the relationship between it and "who" we are is contingent, changing, and simply an analytic distinction (e.g., Nietzsche 1967c, 45). For example, "race" and "gender" are usually regarded as aspects of "what" we are, but their meaning and significance can change over time, depending on how we carry or transfigure the effects they generate (i.e., with "who" we are).

12. The following intellectual cartography could also be extended to map different moments in Hannah Arendt's reflections on action, thinking, and judgment, as well as a variety of interpretive approaches to her writings (as suggested in subsequent notes and citations).

13. Theories of political judgment that draw on a neo-Kantian universalism or *sensus communis* can be analogous to this perspective in important ways, because they tend to efface rather than address the challenges that will-to-power poses to thinking (even when they evade the positivist tendency to conceive "mental faculties" in epistemological terms). When such accounts invoke a universality, "common world," or "shared taste" that are now thoroughly contested through genealogical critique, some may read them as having a nostalgic or coercive air (e.g., Arendt 1958, 50–58; idem 1982; Benhabib 1992, 89–144).

14. Theories of political judgment assume a similar perspective when they emphasize political action's "agonistic" character (e.g., Arendt 1958, 194) without sufficiently articulating responsiveness for the effects of our activities or performances on others and the world. In other words, like its apparent opposite, this perspective must be wary of occluding or inadvertently authorizing the thoughtless exercise of power. While an interpretation deploying Nietzsche and Arendt's "institutionalism" politicizes agonism (Honig 1993, 69–75, 96–125), in my view, we also need to distinguish between "making" or "governing powers" and collaborative, political power (see below) not only to chasten agonism's potential to slide into thoughtless assertion but also to address and avoid the nostalgic and coercive air sometimes associated with the "first camp."

15. Theories of political judgment informed by this third perspective would contain the agonism or virtuosity of performance by invoking a *sensus communis*. In other words, Kant is employed to temper Nietzsche (e.g., Arendt 1978, 1982; Villa 1992). As we shall see, in its effort to reconcile the preceding views, this perspective is subject to the problems of each.

16. To be more precise, what he does tell us usually attributes an essential character to whole classes of human beings in ways that contradict his perspectivism and related philosophical commitments. In my view, Nietzsche mistakenly assumes that the diversity of *bodies* and perspectives can appear without political spaces. This and other matters on which I seek to persuade the reader to follow Arendt rather than Nietzsche—or vice versa—hinge on philosophical issues (ontological and linguistic); for more detailed considerations of these matters than I can pursue here see e.g., Connolly 1991, 1993; White 1991.

17. Arendt relegates all of this to bodies, their "necessities" and "irresistable tendencies," which she regards as without political relevance unless they threaten to intrude upon "the public" (1958, 45–47, 68–73). Arendt does not view the "private," *bodies* and "what" we are as without value, but she does strictly circumscribe their place in politics (their relevance to "who" we are).

18. In consumer society, making has been colonized by laboring (Arendt 1958, 102, 124, 126, 152–53, 157).

19. Arendt would deny the latter point, given her views about the relationship between our *bodies* and political action.

20. I am elaborating Arendt's thinking as it is transfigured by an encounter with Nietzsche. Thus, in what follows I may challenge or contradict some of Arendt's views, especially her "late" formulation (Arendt 1982; see Beiner 1982, 89–94). But my task in this context is not to document her views but to illuminate political thinking.

21. If this seems implausible, consider (recurring to an earlier example) that actually understanding how "what" you are effects others (e.g., the "sort" of person with a history and location that inclines you to speak often and freely while deliberating, which reinforces others' predilection not to do

so) might lead you to alter "who" you are (how you listen to others, when and how you speak). Though the example may seem banal, in my view it would be impossible to overestimate the number of political spaces that have disintegrated or descended into thoughtless assertion when people are not willing to transfigure "who" they are in this or similar ways.

22. This formulation may not be inconsistent with recognizing, hearing, or letting "others" be (e.g., Benhabib 1992; Connolly 1991; White 1991), but it does reflect a different emphasis. In my view, we need to acknowledge the need—perhaps inevitability—of each transfiguring "who" she or he is in response to others (to some extent, Connolly [1993] moves in this direction), because the latter is a condition of the creation and sustenance of political spaces. I understand the centrality given the will-to-power as distinguishing what I advocate from a Gadamerian "fusion of horizons." No doubt, some will find my story still too "moralistic," while others will judge it to be "nihilistic," because insufficiently discriminating.

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RESEARCH NOTES

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AN INDIVIDUAL-LEVEL MULTIEQUATION MODEL OF EXPENDITURE EFFECTS IN CONTESTED HOUSE ELECTIONS

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We shall address questions concerning the impact of candidate spending in congressional elections in a new way. We develop a multiequation model of congressional vote choice that takes the endogeneity of expenditures into account. We then estimate both this model and the more traditional single-equation model using individual level survey data. The substantial differences we find between the two models indicates that the simultaneity bias present in the single-equation model is not trivial. The challenger and incumbent expenditure terms are each significant (whereas only challenger expenditures are in the single-equation setup) and of much greater magnitude in the multiequation case. In addition, we evaluate hypotheses grounded in persuasive communications theory concerning the type of individuals most affected by the messages emanating from the campaigns. We find that individuals with higher education, those with greater interest in campaigns, and those with strongly held convictions are unaffected by candidate spending, whereas individuals lacking each of these attributes are greatly influenced by campaign expenditures.

Since the late 1960s, the amount of money spent by candidates for public office has grown dramatically (Alexander 1992, 82). This spending has generated a corresponding amount of scholarly activity concerned with, among other things, the role that all this money plays in the electoral process. This has been particularly true of congressional races, where most of the efforts to determine the impact of money have been focused (Green and Krasno 1988, 1990; Jacobson 1978, 1980, 1984, 1985, 1990; Ragsdale and Cook 1987; Thomas 1989, 1990). Despite these efforts, the fundamental question of whether increased spending is beneficial to both challengers and incumbents remains unresolved. Intuitively, it seems obvious that spending should enhance a candidate's electoral prospects. Empirically, however, the results are far from conclusive; and recent debates have brought this issue to the forefront again (Green and Krasno 1990; Jacobson 1990).

It is also the case that most of the efforts have been conducted at the aggregate level of analysis; total expenditures for each candidate affect the share of the vote the candidate ultimately receives. Much less work has been done at the level of the individual, so we know relatively little about how the effect of money works, that is, the types of candidates and messages that are most influential and the types of individuals that are most responsive to campaign spending. In addition, individual-level analysis might clarify the inconclusiveness that exists at the aggregate level.

We examine the challenger-versus-incumbent campaign-spending question in a new way. Employing an individual-level analysis of congressional vote choice, we model the relationship between money and vote choice as a system of equations. More specifically, we treat the expenditures as endogenous variables, thus taking into account one of the major points of contention in this debate. In addition, this individual-level design allows us to evaluate hypotheses grounded in persuasive communications theory concerning the type of individuals most affected by candidate spending.

CANDIDATE SPENDING IN CONGRESSIONAL ELECTIONS

Research on the effects of expenditures in competitive congressional elections has focused primarily on aggregate outcomes. Following Jacobson's (1978) seminal article, scholars have attacked the problem in a number of ways. From this body of research, two general results have emerged. First, spending by challengers has consistently been shown to increase their share of the vote. The more challengers spend, the better they do. Second, the same cannot be said of incumbent spending. The evidence in this case is far more conflictual and seems, more often than not, to show that incumbent spending has no effect; that is, increased spending by incumbents does not help them get reelected. This result makes sense empirically, it is argued, because incumbents typically spend large amounts of money in the face of a strong challenge (ibid.).

Many have not been satisfied with this explanation, however, and the bothersome null effect for incumbent spending has generated a great deal of scholarly research. For competitive House races, Jacobson has consistently shown that incumbent spending has no statistically significant effect on the outcome of the election (1978, 1980, 1985). Green and Krasno (1988, 899) argue the opposite and base their conclusions on model misspecification. They show that incumbent expenditures tend to depress the challenger's share of the vote, although their results are difficult to interpret because of a large number of complicated interaction terms and a nonlinear model specification. Ragsdale and Cook (1987) report inconsistent results. Thomas (1989, 1990) also argues that incumbent expenditures matter. He develops a theoretical model and tests it empirically. The results indicate that incumbent expenditures in the 1978 and 1980 elections are positively related (but not significantly so) to the challenger vote share. Clearly, the aggregate analyses have not settled this debate.

Much less work has been done at the individual level. Jacobson considers campaign spending in models of individual familiarity with candidates for House and Senate races (1978, 1980) and preelection vote preference (1990). The latter article includes expenditures for both challengers and incumbents and finds that incumbent expenditures do not affect the outcome. The methodology of Jacobson's 1990 article was contested by Green and Krasno (1990) because of the presence of identifiable measurement error in the expenditure terms. Ragsdale (1981) models the process by which voters evaluate challengers and incumbents in 1978 congressional races. For the House races in the study, she found that evaluations of challengers "negatively affect" the "opinions of incumbents without the reverse being so" (ibid., 201). Her model examines the structural effect of the campaign, including contact by candidates, both through the media and direct campaign efforts and through recognition of the candidate. These factors, for each candidate, are filtered through candidate evaluations and then affect the vote (ibid., 208).

A solid body of theoretical evidence holds that challenger expenditures, incumbent expenditures, and voting outcomes comprise a system of equations (Green and Krasno 1988; Jacobson 1978, 1980, 1984, 1985; Thomas 1989, 1990). Money and electoral outcomes are closely intertwined, it is argued, and any representation of how spending affects outcomes must take this simultaneity into account. A multi-equation model is thus necessary to estimate these relationships correctly. Challenger spending is determined by a set of independent variables. Incumbent spending is a function of challenger spending (and other variables) since incumbents spend in proportion to the challenger they face (Jacobson 1978). Both expenditure terms appear in the outcome equation, as do other independent variables.

In summary, the weight of the theoretical evidence tells us that challenger and incumbent expenditures are endogenous to the voting process and that these terms are themselves functions of other variables. Accounting for this simultaneity at the aggregate level has not cleared up the debate over whether incumbent spending affects electoral outcomes, however. Turning to the individual level does not help because analyses of congressional vote choice typically ignore the effect of spending (Brown and Woods 1991; Kiewet 1983). As a consequence, structural models of individual-level vote choice explicitly incorporating expenditures are not found in the literature. We shall develop a three-equation model of the effect of expenditures on individual congressional vote choice. Estimating a model of this sort requires sets of variables that do not normally appear together in individual-level surveys, however. Information from different sources was used to construct a data set appropriate for evaluating the sorts of questions we are interested in.

THE DATA

Assessing the effect of candidate spending on vote choice requires both individual-level survey information and the campaign expenditures of the congressional candidates who ran in the various districts. We chose the 1984 Inter-University Consortium for Political and Social Research American National (Pre- and Post-) Election Survey (ANES) for the individual level data used in the analyses that follow. These data provide a measure of congressional vote choice as well as standard sociodemographic and attitudinal controls typically used in individual-level vote choice models.

To the survey we added additional information on the congressional candidates, their expenditures, and the setting in which the election took place. Aggregate-level data on campaign spending by the challenger and incumbent running in the respondent's district were gathered from Federal Election Commission reports and added to the survey. Measures of the number of opponents that the incumbent and challenger faced in primary elections, their political party affiliation, and the level of support for the challenger in the previous election were created from information provided in *America Votes* (Scammon and McGillivray 1985). A measure of the number of terms the incumbent had served was constructed from information provided in the *Almanac of American Politics* (Barone and Ujifusa 1986). An objective measure of seat safety and the quality of the challenger were coded from information provided in *Elections 84* (1984).[1]

Given the nature of our investigation (competitive districts with an incumbent running) and the multi-equation specification of the model, some reduction in sample size is inevitable. A description of the subsample of respondents and congressional districts included at each stage of the analysis is reported in the appendix.

THE BASIC MODEL

We investigate directly the impact of challenger and incumbent expenditures on vote choice by estimating the following system of equations:

Challenger Expenditures = f (Challenger Party, Challenger Party Strength, Incumbent Terms in Office, Challenger Political Experience, Challenger Opposition in the Primary, Seat Safety) **(1)**

Incumbent Expenditures = f(Challenger Expenditures, Challenger Party, Challenger Party Strength, Incumbent Terms in Office, Challenger Political Experience, Incumbent Opposition in Primary, Seat Safety) **(2)**

Vote Choice = f(Challenger Expenditures, Incumbent Expenditures, Same Party, Reagan Approval, Economic Expectations, Sex, Race, Education, Age, Income, Presidential Coattails). **(3)**

We diverge from other individual-level analyses in the specification of the vote choice equation (equation 3). The dependent variable in our model measures whether a respondent voted for the challenger or not, rather than candidate familiarity or preelection vote intention (Jacobson 1978, 1990). In terms of the spending variables, we include expenditure terms for both candidates, rather than just for nonincumbents (Jacobson 1978, 480). We also include a standard array of demographic controls, such as the respondent's sex, race, age, education, and family income (the specific codings for the independent variables are included in the tables).

The other independent variables are not quite as well known, and a brief word about their construction is in order. *Economic expectations* is a three-category variable measuring the extent to which respondents believe the economic future of the country is going to improve, remain the same, or get worse. The respondent's rating of the job president Reagan was doing is measured by five categories ranging from −2 (strongly disapprove) to +2 (strongly approve). Related to this measure is a variable we construct to capture a possible coattail effect. This variable is an interaction between Reagan approval and a dummy variable for challenger's party identification. The dummy is coded 1 if the challenger is a Republican and 0 otherwise. This variable ranges from −2 to 2. We expect that respondents who liked the job Reagan was doing will be more likely to vote for a Republican challenger. Finally, a dummy variable measuring whether the respondent identifies with the same party as the challenger or not is included to capture the degree to which party affiliation affects vote choice.

There are basically two ways that expenditure terms have been incorporated into vote choice equations. The first simply uses the aggregate expenditures reported to the Federal Election Commission. Jacobson (1990) employs this method in his single-equation model of individual-level congressional vote choice. As Green and Krasno point out, however, this method ignores the endogeneity of expenditures: "If incumbents spend more because they feel their support slipping, then the individual-level analysis will show that the more incumbents spend, the more their support slips away" (1990, 371). The second method makes use of the theory developed at the aggregate level in a micro model of candidate choice. Instead of actual expenditures, instrumental variables are constructed by regressing the two expenditure terms on all exogenous variables in the system and obtaining the predicted values from each. The predicted values from these first-stage equations are then used in the second-stage vote choice equation.[2] We argue that the single-equation model produces insignificant incumbent expenditure effects because it suffers from the same simultaneity problems of many aggregate models. Estimating a system of equations accounts for the endogenity of expenditures and should produce the intuitively sensible result that spending improves electoral prospects for incumbents as well as challengers, although probably not quite to the same degree.[3]

We investigate this possibility by estimating both the single and multiequation models of congressional vote choice described. First we develop and estimate a standard logit model of vote choice for the House races present in the 1984 postelection wave of the National Election Study (NES). We then estimate the same model again, but instead of actual expenditures we include the constructed instruments for both incumbent and challenger spending described by the structural equations (equations 1 and 2). In addition to reporting tables of these coefficients, we use the model to consider the magnitude of the expenditure effects by holding all other variables constant at their mean values and varying the expenditure terms from zero to one million dollars.

These procedures are employed in Table 1 and Figure 1. The right column of Table 1 contains the results of our two-stage estimation and the left column contains the results of the single-equation estimation. Comparison of the estimated parameters for the expenditure terms across the two models is an illustration of simultaneity bias in the single-equation model. Challenger spending is highly significant and its sign is correct in the single-equation model. Incumbent expenditures in this model, however, appear not to be related to vote choice. In the multiequation model, this counterintuitive result is corrected. The estimates for both challenger and incumbent expenditures are statistically significant and have the proper sign. In addition, Figure 1 shows that these coefficients are severely attenuated in the single-equation model. The effect of spending is much greater in the multiequation model, although challengers gain more from increased spending than do incumbents. These results are clear indicators of the presence of simultaneity bias in the parameter estimates.[4]

The parameter estimates for the other terms are relatively stable. Generally, the same set of exogenous variables are significant in each case. Neither race, sex, education, nor income have crisp statistical effects in either model. Economic expectations fails to meet the test of statistical significance here, though it has been shown to have an impact in other races (Jacobson 1990). Finally, the Reagan approval and coattail variables are strongly significant and properly signed. Individuals who approve the job President Reagan is doing are more likely to vote against the House challenger, unless that House challenger is a Republican, in which case they are more likely to vote for him or her.

TABLE 1

Vote for Challenger by Incumbent and Challenger Expenditures and Respondent Demographics: Single-Equation Versus Multiequation Model

INDEPENDENT VARIABLES	SINGLE-EQUATION MODEL	MULTI-EQUATION MODEL
Intercept	−2.9 (.49)**	−2.4 (.52)**
Same party as challenger[a]	1.6 (.20)**	1.6 (.21)**
Reagan approval[b]	−.54 (.12)**	−.51 (.12)**
Economic expectations[c]	.16 (.14)	.20 (.14)
Sex[d]	.04 (.18)	.08 (.18)
Age[e]	.01 (.006)	.01 (.006)
Race[f]	−.55 (.38)	−.30 (.39)
Education[g]	.07 (.05)	.07 (.05)
Family income 1983[h]	−.02 (.02)	−.006 (.02)
Reagan coattail[i]	.99 (.15)**	.94 (.15)**
Challenger spending[j]	.00196 (.0006)**	.00554 (.0016)**
Incumbent spending[j]	−.00027 (.0006)	−.00425 (.0017)**
Model chi-squared	287.9	290.4

Note: Dependent variable is coded 1 if respondent reported voting for the challenger, 0 otherwise. Logit estimates. Standard errors are in parentheses. N = 938.

[*] $p < .05$.

[**] $p < .01$.

[a] 1 if respondent identifies with the same party as the challenger, 0 otherwise.

[b] Range is from +2 (strongly approve) to −2 (strongly disapprove).

[c] 1 if respondent believes economy will get better, 0 if no change, −1 if get worse.

[d] 1 = female, 0 = male.

[e] Age in years.

[f] 1 = nonwhite; 0 = white.

[g] Range is from 0 (8 years or less) to 10 (advanced degree).

[h] Range is from 1 (less than 2,999) to 22 (75,000 and over).

[i] Interaction between Reagan approval and a dummy variable coded 1 if respondent is a Republican, 0 otherwise. Range is from −2 to +2.

[j] Actual dollars (in thousands) in the first column model; instrumental variable (as described in text) in the second.

Incumbent expenditures matter in election contests for seats in the U.S. House of Representatives. Spending by the incumbent reduces the likelihood that individuals will vote for a challenger and this spending may have a significant impact on the outcome of the race. We now turn to the general question of how this spending effect works.

Campaign Spending as Persuasive Communication

So far we have demonstrated that spending by both challengers and incumbents affects individual vote intention when this relationship is modeled in a theoretically appropriate way. But demonstrating an effect is not the same thing as understanding what the effect means. In the case of campaign spending, the assumption is usually made that this spending represents persuasive communications to the electorate. As Jacobson points out, "basically, then, we have to assume that better-financed campaigns project a higher volume of more persuasive messages, and this is the source of any direct connection we may uncover between money and votes" (1990, 344).

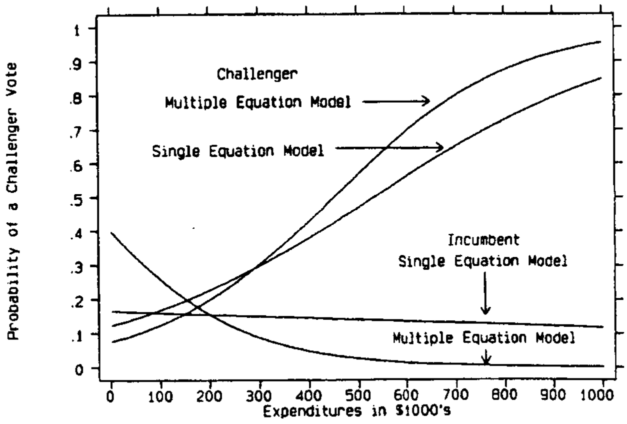
It is perfectly reasonable to interpret the effect of money on vote intention in terms of persuasive communications. Generally, a high percentage of the campaign budget is spent on producing and distributing messages either in favor of the candidate or against his or her opponent (Goldenberg and Traugott 1984). In order more fully to understand the effects of spending in an election campaign, however, we need to evaluate this assumption directly. Several questions concerning the nature of this effect naturally arise. What type of individuals are most or least affected by the messages? What type of candidates are most appealing? Does the type of message have any bearing on its impact?[5]

Fortunately, a great deal of theoretical and empirical work in social psychology attempt to describe the influence of persuasive messages.[6] These studies have identified a number of factors that influence the extent to which a message is persuasive or not. These factors can be grouped into several categories depending on which part of the persuasion process they affect. For example, characteristics of the individual receiving the message (the target) have been found to be important. Individuals with firmly held beliefs are less easily persuaded, as are the more interested and highly educated (Milburn 1991). Certain characteristics of the source of the message have been found to be effective in shaping attitudes as well. The credibility and attractiveness of the source of the message are examples (Lindzey and Aronson 1985). Finally, certain features of the message itself have been shown to affect its persuasive impact. The types of arguments and appeals (e.g., positive or negative), the message style (forcefulness and speed), and the amount of message material (how frequently repeated) are examples (ibid.).

Evaluating the role of each of these factors and potential others would be impossible in the context of this research design. However, the survey-based

FIGURE 1

Single and Multiple Equation Vote Choice Models



design utilized in this study is ideal for examining at least one subset of factors common to any discussion of persuasive communications—the characteristics of individuals receiving the message (the target). Measures of education, interest, and strength of conviction are available in these data and can be utilized to test hypotheses about the types of individuals that are most, and least, affected by the campaign communications. While the other factors are undoubtedly important, we cannot evaluate them in this setting. Essentially, we have to assume they are equal in each case, that is, the candidates are equally credible, attractive, etc.

Individual Characteristics and the Effects of Campaign Spending

Are individuals with more education, those with greater interest in the campaign, and those who hold strong partisan beliefs less susceptible to the persuasive communications emanating from a campaign? In order to evaluate these hypotheses, four interaction terms are created for each estimation of the model. For example, to test the hypothesis that higher-educated individuals are less affected by campaign spending, two variables are set equal to incumbent and challenger expenditures for individuals with a post-high-school degree and equal to zero otherwise in each case. The other two terms are set equal to incumbent and challenger expenditures for individuals with less than a post-high-school degree and equal to 0 otherwise in each case. Interest and strength of conviction are handled in essentially the same way. Expenditure variables for each candidate are created for those who are very much interested in the political campaigns that year, for individuals who identify as strong partisans, and for individuals lacking both of these two conditions. This setup allows us to test for differences in the way money affects different types of people by comparing the coefficients on each of these interaction terms. We expect that the educated, interested, and strong partisans will be much less affected by campaign spending, that is, the coefficients will be smaller and less significant in these cases.

Table 2 and Figures 2–4 show the results when these three estimations are made. The evidence clearly supports our hypotheses. Campaign expendi-

TABLE 2

Vote for Challenger by Incumbent and Challenger Expenditures and Respondent Demographics: Equations with Interaction Terms

INDEPENDENT VARIABLES	EQUATIONS WITH INTERACTION TERMS FOR		
	EDUCATION	INTEREST	STRENGTH OF PID
Intercept	−2.0 (.60)**	−2.2 (.55)**	−2.4 (.54)**
Same party as challenger	1.6 (.21)**	1.6 (.21)**	1.6 (.21)**
Reagan approval	−.52 (.12)**	−.51 (.12)**	−.51 (.12)**
Economic expectations	.20 (.14)	.20 (.14)	.20 (.14)
Sex	.08 (.18)	.07 (.18)	.08 (.18)
Age	.008 (.006)	.009 (.006)	.009 (.006)
Race	−.24 (.39)	−.30 (.39)	−.31 (.39)
Education	.016 (.065)	.07 (.05)	.07 (.05)
Income	−.004 (.02)	−.006 (.02)	−.006 (.02)
Coattail	.95 (.15)**	.93 (.15)**	.94 (.15)**
High education[a]	.003 (.70)	—	—
Interest[b]	—	−.52 (.58)	—
Strength of party ID[c]	—	—	−.11 (.64)
Challenger$ × high[d]	.0010 (.0027)	.0036 (.0024)	.0047 (.0029)
Challenger$ × low[e]	.0077 (.0019)**	.0067 (.0020)**	.0058 (.0018)**
Incumbent$ × high[f]	−.0016 (.0030)	−.0021 (.0026)	−.0036 (.0032)
Incumbent$ × low[g]	−.0060 (.0020)**	−.0056 (.0021)**	−.0045 (.0019)*
Model chi-squared	299.8	291.7	290.5

Note: Dependent variable is coded 1 if respondent reported voting for the challenger, 0 otherwise. Logit estimates. Standard errors are in parentheses. N = 938.

*$p < .05$.

**$p < .01$.

[a] 1 if respondent completed a college degree of some sort; 0 otherwise.

[b] 1 if respondent reported being very much interested in the political campaigns of 1984, 0 if somewhat or not much interested.

[c] 1 if respondent identifies strongly with one of the two parties, 0 otherwise.

[d] Challenger expenditures (educated, interested, strong partisans). Col. 1: challenger expenditures for respondents who score 1 on high education variable, 0 otherwise. Col. 2: challenger expenditures for respondents who score 1 on interest variable, 0 otherwise. Col. 3: challenger expenditures for respondents who score 1 on strength of party ID variable, 0 otherwise.

[e] Challenger expenditures (less educated, less interested, not strong partisans). Col. 1: challenger expenditures for respondents who score 0 on high education variable, 0 otherwise. Col. 2: challenger expenditures for respondents who score 0 on interest variable, 0 otherwise. Col. 3: challenger expenditures for respondents who score 0 on strength of party ID variable, 0 otherwise.

[f] Incumbent expenditures (educated, interested, strong partisans). Same coding for incumbents as described for challengers in n. d.

[g] Incumbent expenditures (less educated, less interested, not strong partisans). Same coding for incumbents as described for challengers in n. e.

FIGURE 2

Probability of Voting for the Challenger, Expenditures Interacted with Education Categories

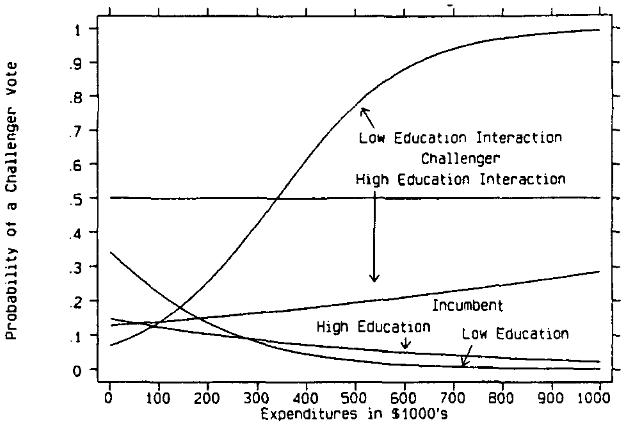
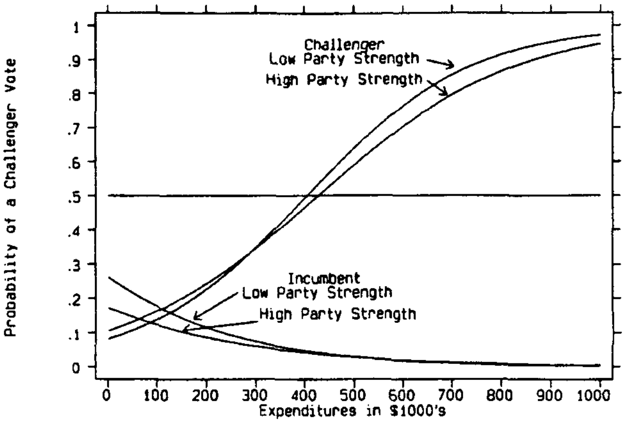


FIGURE 4

Probability of Voting for the Challenger, Expenditures Interacted with Party Strength Categories



tures affect individuals in precisely the way that theories of persuasive communication would predict. Table 2 (column 1) and Figure 2 show that individuals with college degrees are unaffected by spending from either the incumbent or the challenger, whereas those without degrees exhibit very large and significant effects in each case (compare with the baseline result in Table 1, col. 2). Regardless how we interpret this result (skills learned from schooling or traits brought to bear on the education process in the first place), it seems clear that highly educated individuals base their vote decision on factors other than the campaign messages that result from spending money.

Table 2 (column 2) and Figure 3 show the results when interest in campaigns is considered in interaction with the spending variables. Again, individuals very much interested in political campaigns are much less affected by campaign spending than are those who are only somewhat or not much interested. Less interested individuals exhibit large and significant effects when compared with their more interested counterparts. Although the differences are not quite as pronounced as they were in the case of education, they are still noticeable. Interested individuals undoubtedly find plenty of other sources of information about candidates and do not rely on campaign communications as greatly as less interested individuals.

FIGURE 3

Probability of Voting for the Challenger, Expenditures Interacted with Interest Categories

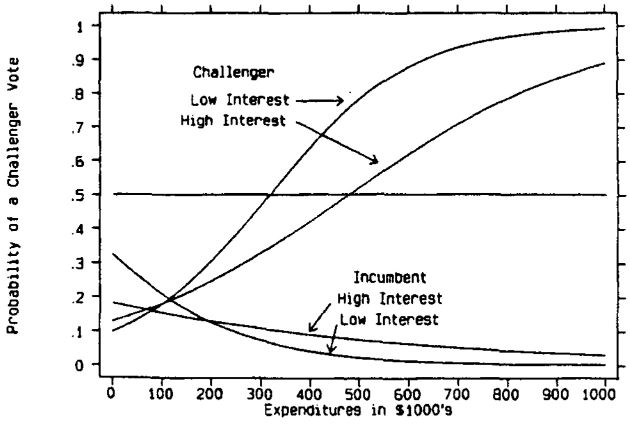


Table 2 (col. 3) and Figure 4 show the results when strength of partisanship is considered in interaction with the expenditure variables. Once again our expectations are supported. Strong partisans are less affected by spending than are weaker partisans, although the differences are not as great as in the previous two cases. Firmly held beliefs are harder to influence and these results clearly support this notion. Candidates have operated under this assumption for some time, making a pitch for the wavering voters an important part of their campaigns.

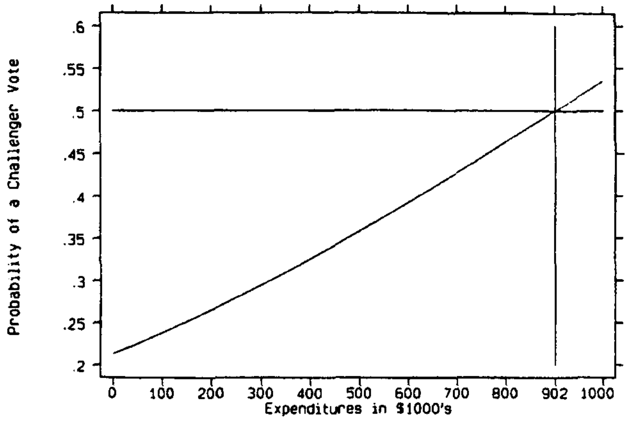
SUMMARY AND DISCUSSION

We have argued that the single-equation model of congressional vote choice incorporating actual candidate expenditures is misspecified. As an alternative, we develop a multiequation model in which expenditure variables are endogenous. Our results indicate that the estimates in the single-equation case are biased and inconsistent. Both the challenger and the incumbent expenditure terms are significant in the multiequation case, whereas only challenger spending achieved significance in the single-equation estimation. In addition, the magnitude of these effects is much greater in the multiequation model.

We have also attempted to determine the types of

FIGURE 5

Probability of Voting for the Challenger When Both Candidates Spend Identical Amounts



individuals most affected by these campaign expenditures. Typically, the assumption is made that voters' decisions are not affected by the money per se but rather, that money represents persuasive communications to the electorate, so more money means greater persuasive ability. Deriving our hypotheses from persuasive communications theories, we find that voting behavior is conditioned by the interaction between spending and particular individual characteristics such as education, interest in campaigns, and the strength with which attitudes are held. Respondents with college degrees are unaffected by candidate spending, whereas those with less than a college degree exhibit very large and significant effects. The same pattern emerges for respondent interest in the campaigns and strength of partisanship, although these affects are not quite as pronounced as in the case of education.

These results are not strictly comparable to those reported in the aggregate studies because we operate at a different level of analysis (see Green and Krasno 1988, 1990; Jacobson 1978, 1980, 1985, 1990). They do, however, touch on the main points of contention in the campaign spending literature. First, spending benefits incumbents as well as challengers, although not quite to the same degree. The probability that an individual will vote for each candidate increases as that candidate spends more money. These results support the contentions of, most notably, Green and Krasno (1988, 1990), who argue that incumbents, as well as challengers, benefit from campaign expenditures.

Second, these results may help inform decisions on certain policy recommendations designed to reduce the advantages of incumbency, such as public financing with spending limits for incumbents and challengers. Figure 5 shows the probability of a vote for the challenger when spending by each candidate is equal. The procedure is similar to that employed in Figures 1–4, except that both expenditure terms are allowed to vary at once. It is clear from this figure that incumbents are advantaged at equal levels of spending until expenditures reach $902,000 (1984 dollars). It is not until expenditures reach this level that the probability of a vote for the challenger reaches .5, that is, equal spending produces an equal chance of receiving a respondent's vote. While this result seems to suggest that relatively high public assistance and spending limits are necessary to achieve parity, it does not mean that limits and assistance at lower levels could not greatly improve challenger chances (as Figure 5 shows), especially given that challengers typically spend far less than that proposed in recent spending limits legislation.

Finally, the additional knowledge of who is most and least affected by campaign spending suggests that campaigns can perhaps target their resources to an even greater degree than they do now. A common statement made by campaign strategists is that half the money they spend is wasted, the problem being they do not know which half. Our results begin to address this question by describing characteristics of individuals who are unaffected by the expenditures. Table 2, column 3, for example, suggests the overlap (hence loss of efficiency) between spending by political parties to strengthen ties and spending by candidates to influence preference. In general, Table 2 suggests that the individuals most responsive to the campaign spending are also those least likely to vote, since they share many of the same attributes (low interest, low education, and weak partisan ties). A prior analysis has shown that campaign spending, at least for challengers, has its greatest effect on individuals who do not vote (Kenny and McBurnett 1992, 935). In light of this, candidates might consider stepping up efforts to stimulate turnout among groups that traditionally participate at low levels, since a committed person who does not vote does not help the cause.

APPENDIX: DESCRIPTION OF SUBSAMPLES OF 1984 AMERICAN NATIONAL ELECTION STUDY USED IN THE ANALYSES

The data set used in the analyses was constructed by merging campaign-specific information gathered from various sources (see data description in text) with the 1984 ANES study. This procedure created a data set of 2,257 respondents residing in 134 districts. Estimation of the first-stage equations reduces these numbers to 1,368 respondents residing in 103 districts. This reduction occurs because certain respondents have missing values for variables included in the estimations. For the most part, excluded respondents reside in districts that either were not competitive (no challenger) or were for open seats (no incumbent) in 1984 and so are missing on variables such as *challenger quality* and *challenger prior political support*. Of the 1,681 respondents residing in competitive districts with an incumbent running, 313 are missing on some other exogenous variable, most commonly the income and/or economic expectations variables. Estimation of the second-stage vote equation reduces these numbers to 938 respondents residing in 96 districts. This reduction occurs because 430 of the 1,368 respondents who survived the first-stage estimations are missing on the congressional vote variable, either because they did not vote at all or because they voted for president but did not vote in the House race.

Fortunately, the distributions on key variables do not change much as the sample size is reduced. Mean challenger spending was around $117,000 in the 103 competitive districts (first-stage equations) compared to $118,000 in the 96 districts used in the second-stage estimations. For incumbent spending, these numbers are $275,000 and $273,000, respectively. In terms of vote choice, 25.6% of respondents who reside in the 103 competitive districts voted for the challenger, compared to 26.0% of respondents in the 96 districts used in the second-stage vote equation.

Notes

We wish to thank Charles Franklin for his helpful comments on an earlier version of this manuscript.

1. Questions may arise as to whether the 1984 ANES study was designed to give representative samples from congressional districts. It is our contention that the sampling within districts is not relevant for the estimation of our model. We have a national sample of voters who were exposed to various campaign stimuli (money) and had personal characteristics relevant to voting (party identifications, etc.). We do not aggregate individuals within districts to estimate district characteristics but rather, estimate an individual-level model of vote choice in which one element of the choice is the candidate spending. In order to estimate the model we need (1) variation in spending across voters (which we have), (2) an assumption that spending (and all other effects) are constant across districts (possibly debatable but certainly the normal practice in modeling congressional voting), and (3) correct model specification. Therefore, whether the sample is representative of the district or not is irrelevant for the analyses we conduct. For an early discussion of this question, see Miller and Stokes 1966.

2. This is a standard method of purging any correlation between the instruments and the error terms in the equations. The presence of any such correlation produces inconsistent and biased estimates (Johnston 1984). Bartels explicitly addresses the appropriateness of instrumental variables in a model similar to that used here (1991, 791–93). His model uses aggregate data and does not improve on previous 2SLS estimates of incumbent expenditures. (The t-values indicate that, at worst, the estimate in his model for incumbent spending is zero, and at best, the estimate is inefficient.)

3. Many studies have argued that campaign expenditures are subject to diminishing returns and that variables measuring spending should be transformed in some way. We argue that diminishing returns in the expenditure variables are captured in the logit estimations of the equation, which forces the expenditure effects onto the 0, 1 interval. We also estimated the model using the customary logarithmic transformation of the expenditure variables and found little substantive difference from the reported results. The coefficient for incumbent expenditures decreases slightly in significance ($p < .10$) but remains properly signed.

4. Since our individual-level analysis uses a different dependent variable (vote choice) than has been used in the few previous studies of this sort (preelection candidate preference and candidate familiarity), the reader might be interested to know what difference this made in and of itself. Unfortunately, there is no preelection candidate preference measure in the 1984 ANES (or any other year for that matter) study so a comparison with Jacobson's (1990) results is not possible. When a measure of candidate familiarity is included as a dependent variable (instead of vote choice) in the estimation, the coefficient for challenger spending is significant, but incumbent spending is not. This result holds for both the single and multiequation formats. These results are consistent with those obtained by Jacobson (1978).

5. Obviously, there are other factors that may mediate the effect of spending in congressional campaigns. For example, the medium through which the voter is reached (television, mail, personal contact, etc.) may influence the impact of the message. We take this question up in a separate effort (Kenny and McBurnett 1993).

6. For reviews of this literature, see Lindzey and Aronson 1985; Milburn 1991; Smith 1982.

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CONTROVERSY

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POPULAR INFLUENCE ON SUPREME COURT DECISIONS

In their 1993 article in this Review, *William Mishler and Reginald Sheehan reported evidence of both direct and indirect impacts of public opinion on Supreme Court decisions. Helmut Norpoth and Jeffrey Segal offer a methodological critique and in their own reanalysis of the data find, contrary to Mishler and Sheehan, no evidence for a direct path of influence from public opinion to Court decisions. Instead, they find an abrupt-permanent shift of judicial behavior consistent with an indirect model of influence whereby popularly elected presidents, through new appointments, affect the ideological complexion of the Court. In response, Mishler and Sheehan defend the direct public opinion linkage originally noted, at both individual and aggregate level; respond to the methodological critique; and offer further statistical analysis to support the aggregate linkages.*

COMMENT

Among the most important questions that can be asked about the Supreme Court is the role of this unelected and democratically unresponsive institution as a policymaker in the national political system. Supporters of the Court usually make one of two defenses of the institution. First, in what is commonly known as the Madisonian response, supporters claim that the Court is not supposed to be a democratic institution. Its raison d'être is to check the excesses of legislative majorities. The second response is of more recent lineage, tracing its history not to James Madison but to Robert Dahl (1957). According to Dahl, it simply does not matter very much if the Court is unelected and thus procedurally undemocratic because it is responsive to national majorities and thus substantively democratic.

Dahl's explanation for Supreme Court responsiveness was quite simple: the people elect the president and the president selects the justices with the advice and consent of the Senate. Thus, with the exception of the onset of critical or realigning periods, the president's appointment power should ensure a Court whose views represent those of the dominant political coalition.

Mishler and Sheehan (1993b) take Dahl's argument a step further. They argue that Supreme Court decisions not only correlate with public opinion but are *directly* influenced by public opinion. According to Mishler and Sheehan, the justices "are broadly aware of fundamental trends in ideological tenor of public opinion, and that at least some justices, consciously or not, may adjust their decisions at the margins to accommodate such fundamental trends" (p. 89). After presenting a variety of time-series models, they argue that Supreme Court justices between 1961 and 1981—but not thereafter—were influenced by whatever levels of liberalism existed in public opinion *five years earlier.* No significant influence is found for contemporaneous public opinion, public opinion lagged at less than five years, or public opinion lagged at more than five years. Because of what we consider to be the questionable nature of their main finding, we decided to investigate the purported relationship further.

We first discuss the theoretical reasons why it is unlikely for Supreme Court decisions to be directly swayed by public opinion, especially at substantial lags. (This is not to say that Court decisions may not correspond to public opinion through other pathways.) We then conduct a methodological audit of Mishler and Sheehan's time-series analysis. One of the problems is that the authors perform an aggregate-level analysis on an individual-level theory. As we shall show, an examination of the behavior of the individual justices provides little support for their claims. Moreover, their aggregate analysis overlooked a sizable shift of the Court series in 1969–71. That change, corresponding to the transition from the Warren Court to the Burger Court, seriously confounds the estimation of popular influence on Court behavior. When the Court realignment is introduced into the analysis, the effect of public mood vanishes. The same happens when we conduct separate estimations for the Warren era and the Burger–Rehnquist era. The relationship also disappears when we control for contemporary Court ideology, rather than (as Mishler and Sheehan do) lagged ideology. Whatever influence public opinion exercises on the Court occurs indirectly, through the choice of justices by presidents chosen by the people.

SUPREME COURT RESPONSIVENESS TO PUBLIC OPINION

We recognize from the outset that Supreme Court decisions might correlate with public opinion. Dahl's presidential appointment hypothesis is convincing. Indeed, numerous scholars have found that the Court is not generally out of line with public opinion. The most exhaustive examination of this issue to date found that out of 130 Supreme Court decisions that could be matched with public opinion polls, 82 (63%) were consistent with poll pluralities (Marshall 1989, 78). Mishler and Sheehan find a contemporaneous correlation of .56 between public opinion and Supreme Court decisions, though this drops to $-.01$

once the autocorrelation of both series is removed. Whatever the real-world correlation might be, it is unlikely to be the result of a direct influence of public opinion on the Court.

The public has substantial influence over the votes cast by members of Congress (e.g., Bartels 1991; Fiorina 1974; Kalt and Zupan 1984; Miller and Stokes 1963). This, of course, is because they are electorally accountable. Supreme Court justices, with life tenure, have no personal incentives to allow such influence. If, in fact, they do so, it is because they feel that they ought to (Gibson 1991).

If the justices were behaving in such a civic-minded fashion, one might imagine that they would advertise such actions to their public. Occasionally, statements in their opinions do suggest the importance of public opinion. Mishler and Sheehan bolster their theoretical claim that the Court is influenced by public opinion with one such quote from Justice Frankfurter and another by Justice Rehnquist (1993b, 89). A more systematic analysis, however, demonstrates how rare such claims are. Public opinion is mentioned in less than 2% of majority opinions (Marshall 1989, 35–36), and in only one-fifth or so of the cases in which it is mentioned do the justices suggest that law should reflect public opinion (ibid., 35–39).

In certain substantive areas, especially death penalty cases, the Court does discuss the importance of public opinion with a frequency greater than nil, but the justices' misuse of public opinion polls there shows how little regard they in fact have for that opinion. Justice Marshall, in *Gregg* v. *Georgia* (1976, 232) argued that the Court should be guided not by what public opinion polls say but by what they would say if the public were fully enlightened on the particular subject. On the flip side, Justice Scalia, speaking for the Court in *Penry* v. *Lynaugh* (1989, 289) argued that public opposition to the death penalty for the retarded is not proven by the fact that the citizens of Texas, Florida, and Georgia—three states overwhelmingly in favor of the death penalty—opposed such executions by majorities of 73%, 71%, and 66%, respectively.

More typical, we presume, of the justices' beliefs, is the view of Justice Jackson: "The very purpose of the Bill of Rights was to withdraw certain subjects from the vicissitudes of political controversy, to place them beyond the reach of majorities and officials and to establish them as legal principles to be applied by the courts. One's right to life, liberty and property, to free speech, free press, freedom of worship and assembly, and other fundamental rights may not be submitted to vote; they depend on the outcome of no elections." (*West Virginia Board of Education* v. *Barnette* 1943, 638. The overwhelming indifference of the justices' opinions to public opinion supports the perceived validity of Jackson's statement.[1] It is possible, nonetheless, that the justices react to public opinion even though they may not be aware that they do so, which is where political science enters the picture.[2] Empirical research on this question is far from complete (that is no doubt why Mishler and Sheehan embarked on their study), but the evidence that does exist shows that while elected state court judges respond to public opinion (Brace and Hall 1990; Gibson 1980; Kuklinski and Stanga 1979), life-tenured federal court judges show no such influence (Giles and Walker 1975; Songer, Segal, and Cameron 1994).[3]

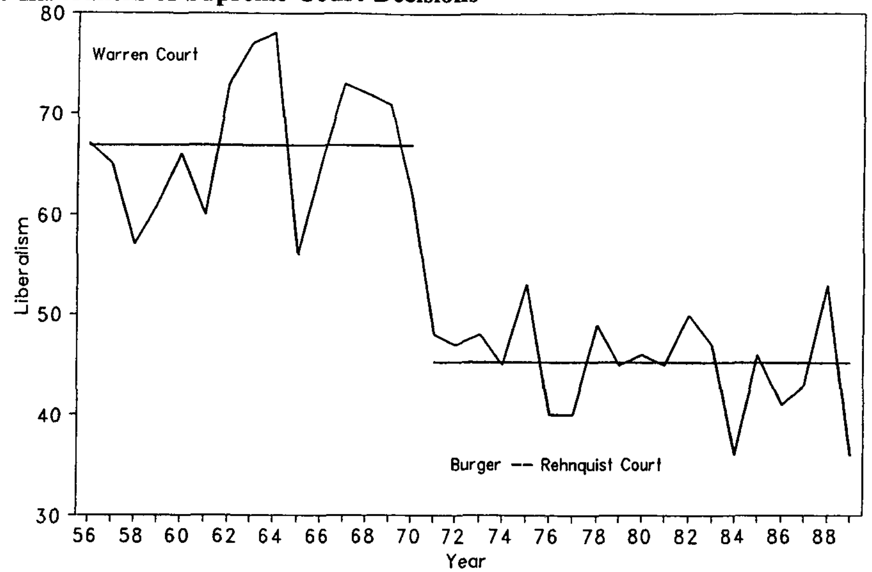
Despite this evidence, let us grant the possibility that the Court might be directly influenced by public opinion. Even if it were true, we find it extremely difficult to accept that the Court is influenced by public opinion that is five years old, regardless of the direction that opinion has taken in the intervening years. Yet that is the claim that Mishler and Sheehan make. Their justification is as follows: "Even where the Court responds directly to perceived changes in public opinion as proposed by the political adjustment hypothesis, it still takes time for justices to perceive a change in public mood. More importantly, it takes time for societal trends to be absorbed and reflected in the attitudes and beliefs of the justices or for justices to become convinced that the change is of sufficient salience, magnitude, and endurance as to threaten the Court's authority" (1993b, 91–92).

It is hard to state precisely what one would predict about judicial behavior from such a statement. If it takes a significant amount of time for the justices to learn of the public mood, then there conceivably might be distant lags without contemporaneous ones, as in the five years that Mishler and Sheehan find. But if the justices need to be convinced that the change is salient enough to threaten the Court's authority, then it must be the case that the justices perceive that the change has endured, for there could not be a threat to the Court's authority from public opinion five years removed. There are two contradictions here. First, if the Court only acts on change that has endured, their decisions should be influenced by contemporaneous as well as lagged public opinion. Yet Mishler and Sheehan find no support for contemporaneous opinion. Second, since it takes long periods of time for the justices to learn about changes in public mood (a dubious assumption in and of itself), the justices would never know whether any change five years old had endured to the present.

The theoretical plausibility of the influence of a five-year lag might best be assessed by trying to imagine the internal processing—be it conscious or subconscious—that would lead to such behavior. A justice might, for instance, think: "X is the decision that I prefer (or that I believe the Constitution requires). But public opinion is much more conservative on this issue than I am. Because we are, but should not be, an undemocratic institution, I'll give the public the benefit of the doubt and vote Y." What we find far more questionable is that a justice would say: "X is the decision I prefer, but public opinion five years ago (but not necessarily any longer) was much more conservative on this issue then than I am now. Because we are, but should not be, an undemocratic institution, I'll give the public what it wanted five years ago." To ask whether a justice could think this

FIGURE 1

A Measure of the Liberalism of Supreme Court Decisions



way is to answer the question. In that case, however, we must ask how Mishler and Sheehan could have amassed such convincing evidence for a lagged effect of mass opinion.

THE FIVE-YEAR LAG

It is important to note that the evidence comes from an analysis of time-series data. Time, as one sage put it, is a great teacher, but it is also a mischievous one, who loves to play tricks on its students. Over time, many things rhyme for no reason whatsoever, like Super Bowl victories and stock market rallies. This tends to occur rather frequently when observations are in short supply. In their analysis of Court behavior, Mishler and Sheehan work with only 34 observations; that number effectively shrinks to 29 when the public opinion series is lagged five years. Tests of statistical significance may afford some protection against falling victim to artificial relationships, but time, again, has its ways of disarming those alarms. This is especially true when one searches a large number of lags, as Mishler and Sheehan do. While the probability of falsely finding a significant lag for any particular lag is .05, the probability of falsely finding at least one significant lag when one searches lags 0–10 is $1-.95^{11}$, or .43.[4]

To be sure, Mishler and Sheehan are wary of the pitfalls of time series and display great care in avoiding many of them. In particular, they identify the five-year lag from ARIMA-prewhitened cross correlations and control for serial dependence of errors in all their structural estimations. They also test for parameter stability over time and make a substantial adjustment as a result of that test. Nonetheless, Mishler and Sheehan make some judgment calls (or, better, noncalls), with game-breaking consequences in their time-series analysis.

In a nutshell, the authors treat the Supreme Court during the 1956–89 period as one when it is really two very different courts: a Warren Court and a Burger–Rehnquist Court. The decisions handed down by those two courts have little ideological ground in common. The contrast is especially sharp when (as in the analysis at hand) annual aggregates are used. Figure 1 plots those annual values, as graciously provided to us by Mishler and Sheehan. There is not a single year in which the Warren Court comes out on the conservative side of the mean level for the whole period and not a single year in which the Burger–Rehnquist Courts wind up on the liberal side.

At the very least, such behavior violates the assumption of mean-stationarity. A series is stationary in the mean if its realizations derive from a distribution with a fixed central tendency (Granger and Newbold 1986, 4). That does not hold true for the Court behavior series from 1956–89. Instead, the outcomes of the Warren Court fluctuate around one ideological level and those of the Burger–Rehnquist Court around another one. Failure to control for this gap is bound to confound the effect attributed to any explanatory variable (however plausible) and to give rise to spurious dynamic effects.

A simple way to control for the sharp contrast in Court behavior is to inquire whether the hypothesized relationship between public mood and Court behavior can be confirmed for each Court separately. For that purpose, Table 1 presents correlations up to five lags between Court liberalism and public mood for the Warren Court and the Burger–Rehnquist Court. As can be seen, the correlations at lag 5 are small and insignificant for both Courts, nor are any other correlations significant. There is no evidence

TABLE 1

Correlations between Public Mood and Supreme Court Decisions by Court Era

	SUPREME COURT DECISIONS	
PUBLIC MOOD	WARREN ERA	BURGER–REHNQUIST ERA
Unlagged	.03	−.13
Lagged one year	.10	.05
Lagged two years	−.04	.03
Lagged three years	.06	.05
Lagged four years	.25	.14
Lagged five years	.24	.10
Standard errors	(.26–.32)	(.23)
Number of cases	10–15	19

Note: Correlations are the Pearson correlation coefficient (R). For the Warren era, the standard errors grow with increasing lags because of a shrinking number of available cases.

that either the Warren Court or the Burger–Rehnquist Court was responsive to variations of the public mood, no matter what the time lag.

Instead of splitting up the time series, we can also reexamine the relationship between public mood and Court behavior by adding a specification for the transition from Warren Court to Burger–Rehnquist Court. For comparison's sake, Table 2, column 1, reproduces results for the whole Court series in the absence of such a specification. Those results show a significant effect for public mood lagged five years as well as for the autoregressive error structure. But these estimates change dramatically when the transition from Warren Court to Burger–Rehnquist Court is introduced. This *Court realignment* (as we have called the transition) shifted the balance of Court behavior from liberal to conservative in the manner of an abrupt–permanent intervention (Box and Tiao 1975; McDowall et al. 1980). The control for this intervention wipes out the apparent effect of public mood on Court behavior. In addition, the control exposes the errors as being purely random, not first-order-autoregressive, as specified by Mishler and Sheehan. That specification also proves to be an artifact of overlooking the Court realignment.

How, then, the patient reader will ask, could the estimate for a five-year lagged effect of public mood arise? The answer is that exactly five years prior to the Court realignment the mood of the American public, as captured by the Stimson measure, must have undergone a swing of staggering dimension. That is indeed what happened. In 1965–66, the mood series falls from a high point of liberalism to a deeply conservative level. The tandem movement of the Supreme Court five years later is enough, with fewer than 40 observations, to create the appearance of a five-year lagged effect of public mood. As demonstrated in Table 2, were it not for this singular parallel in the two series, no finding of an effect with a five-year lag would have emerged.

Granted, there is some broad semblance between the contours of public opinion and Court behavior, with the generally liberal Court decisions of the Warren era corresponding to a like-minded public mood and the more conservative decisions of the Burger–Rehnquist era doing so for a more conservative public. Nonetheless, compared with Court behavior, the public is far more "moody," shifting frequently from liberal to conservative and vice versa. There is no evidence that the Court pays any attention to all those mood swings of the general public, either immediately or five years later. What remains is that in one singular incident a change of Court behavior duplicated a shift in public opinion five years earlier.

Whatever the reasons may have been for the public mood swing in the mid-1960s, there is little mystery about what changed the Court's behavior in the early 1970s. Within barely two years of taking office in 1969, President Nixon was able to replace four of the nine justices, every one of the newcomers being decidedly more conservative than his predecessor. In their first year of service, the new members (Burger, Blackmun, Powell, and Rehnquist) averaged a liberalism score of 29 compared with an average of 64 for their predecessors (Warren, Fortas, Black, and Harlan) in their last year.[5] A "generational replacement" of such volume and contrast is surely capable of accounting for the Court's ideological realignment in the early 1970s.

Among the remaining justices of the Warren Court, Douglas, Brennan, and Marshall remained loyal to their liberalism, and Stewart to his moderate position. As the correlations for individual justices in Table 3 indicate, the public's mood had no significant bearing on the decisions of the three liberals, regardless of the time lag.[6] For Justice Stewart, we detect a statistically insignificant hint of some instan-

TABLE 2

Supreme Court Decisions as a Function of Public Mood Lagged Five Years and Court Realignment

INDEPENDENT VARIABLES	COEFFICIENTS I	COEFFICIENTS II
Constant	53.21** (3.63)	66.21** (2.04)
Public mood (t − 5)	.61** (.26)	.19 (.15)
Court realignment	—	−19.92** (3.00)
AR(1)	.60** (.17)	−.09 (.19)
Ljung–Box Q(8)	2.9	4.8

Note: Standard errors are given in parentheses. Court Realignment is a binary variable scored 0 for the years 1956–70, and 1 afterward. The public mood series was centered before model estimation to make the constant more interpretable. AR is the autoregressive parameter.
$^{*}p < .05$.
$^{**}p < .01$.

TABLE 3

Correlations between Public Mood and Decisions of Individual Justices

Public mood	Decisions of Douglas	Brennan	Marshall	Stewart	White
Unlagged	.03	.16	.21	.39	.15
Lagged one year	.06	.16	.16	.32	.23
Lagged two years	.11	.20	.23	.24	.32
Lagged three years	.23	.16	.08	.24	.27
Lagged four years	.25	.17	.07	.29	.46*
Lagged five years	.18	.18	.10	.14	.47*
Standard errors	.22–.26	.17–.19	.21–.24	.20–.23	.19–.21
Number of cases	18–23	29–34	18–23	19–24	23–28

Note: Correlations are the Pearson correlation coefficient (R). The standard errors grow with increasing lags because of a shrinking number of available cases.
*$p < .05$.

taneous effect, but nothing with a delay of five years. Only one justice *appears* to exhibit in his individual behavior what the Court underwent as a whole. There exists a bivariate correlation between Justice White's votes and public opinion five years earlier. And like the Court as a whole, this justice shifted from liberal to conservative during the transition years. But whether his conversion can be attributed to the public's turn toward conservatism five years earlier is doubtful. After statistically controlling for his abrupt-permanent shift, White shows no evidence of responding to public mood. Alternatively, prewhitening renders the lagged correlations at four and five years insignificant. Further evidence is provided by the fact that he has shown little inclination to follow other mood swings of the general public, especially the liberalization of the 1980s. Why would he have reacted to public opinion then, but not more recently? White's conservative shift in the early 1970s could be just as plausibly (if not more so) attributed to a new conservative president (Epstein, Walker, and Dixon 1989), a new conservative Court and/or chief justice (Murphy 1964), or simply to changes in his own preferences that were totally uninfluenced by changes in public preferences half a decade earlier.

To be fair, let us acknowledge that Mishler and Sheehan are not oblivious to the change in the Court's ideological composition. Indeed they control for a compositional effect by introducing a measure of ideological preferences designed by Segal and Cover (1989). While the addition of a Court membership variable does attenuate the effect of public mood, Mishler and Sheehan are still able to report significant results. It is not a trivial feature of this control, however, that Court membership enters the analysis with a lag of one (year). In other words, the authors suppose that the Court's decisions this year derive from the Court's ideological complexion last year. But why last year and not this year? In particular, how telling are last year's ideological inclinations for this year's decisions when the membership of the Court has greatly changed? As Table 4 shows, when ideological composition enters the analysis unlagged, the effect attributed to public mood disappears.[7]

In an important way, Mishler and Sheehan themselves qualify the finding that the Supreme Court is responsive to public opinion. The authors note that "the ideology of the public mood and the decisions of the Court have diverged rather sharply since President Reagan's election in 1980" (1993b, 94). To accommodate this divergence they include an interactive term for public mood during those years. The estimate for this interaction is negative, while the estimate for mood across the whole period is positive. Taken together, this suggests that during roughly one-third of the period examined, public opinion had little effect on Court behavior. But why did the Court suddenly prove unresponsive to public opinion? There is no evidence that the Court's ideological tenor changed in 1981. What did change was public opinion. Surprisingly, Stimson's mood measure swings toward the liberal side during the Reagan years. It is a turn that the Court did not follow in its decisions. But if the Court appears free to ignore the public mood at one moment, it may be simple coincidence that its decisions square with the public mood at another moment.

In short, Mishler and Sheehan's aggregate results are seriously confounded by a violation of the mean-stationarity assumption (to put it technically). But even if we ignored this assumption, their estimates would have proved insignificant had they controlled for contemporary Court ideology, not lagged Court ideology. Moreover, even without this control (and again ignoring the stationarity problem), the results they provide are entirely due to a shift in public mood in 1965–66 that coincided with a change of Supreme Court voting behavior in 1970–71. That result, as our

TABLE 4

Supreme Court Decisions as a Function of Lagged Public Mood and Contemporaneous Court Composition

INDEPENDENT VARIABLES	COEFF.	S.E.
Public mood ($t - 5$)	.09	(.19)
Court composition (t)	3.29*	(.59)
AR(1)	$-.35 \times 10^{-2}$	(.19)
Ljung–Box Q(8)	5.1	

Note: All variables were centered prior to model estimation—hence, no constant. N = 34. AR is the autoregressive parameter.
*$p < .01$.

individual-level analyses demonstrate, is virtually entirely due to changes in the Court's composition. Finally, the authors' extensive search for lags ignores the corresponding increase in the probability of a type I error.

DISCUSSION

Does public opinion influence Supreme Court decisions? If the model of influence is of the sort where the justices set aside their own preferences and abide by what they divine as the *vox populi,* our answer is a resounding *no*. In particular, we find no evidence that such an influence occurs with a delay of five years. What we do find, however, is that a sizable ideological shift in Court behavior took place in 1970–71 in the wake of an unmistakable swing of public opinion roughly five years earlier. This might be pure coincidence. With no more than one incident of this kind in the data available, caution may be well advised. On the other hand, this incident may be consistent with a model of influence, but of a very different sort.

While justices are not accountable to the populace, presidents and senators, who share the power to choose them, are. Whatever configuration of public opinion elects a president, in particular, could transpire in his or her Court appointments. Given plentiful opportunity to put new members on the Court, a newly elected president can alter the Court's ideological complexion. It is not that the justices pay keen attention to public opinion but that they have been chosen by a president (with the advice and consent of the Senate) who presumably shares the public's views. If, however, the president is elected on the basis of issues orthogonal to those decided by the Court or in the absence of voter concern for ideological issues, then there is no reason to believe that his or her appointees will reflect public opinion in their judicial opinions. This is the crucial difference, from a responsiveness standpoint, between justices who are directly (versus indirectly) influenced by public opinion. Supreme Court responsiveness will not necessarily exist but will be dependent on the nature of presidential elections. For instance, if Ronald Reagan had been elected in 1980 because of the economy and foreign affairs and not his conservative social agenda, then we would expect little match between public mood and Court decisions in the 1980s. Alternatively, if we assume that the public's mood swing in the mid-1960s helped carry Richard Nixon to the presidency, we may be able to credit the public with an indirect hand in the Court's shift to the right in the early 1970s. By 1971, Nixon's appointments of new justices had definitely shifted the ideological balance of the Supreme Court to the conservative side. This shift was reinforced during the next 20 years, since all vacancies on the Court were filled by Republicans in the White House. Every quarter century or so, it seems, public opinion has its day in court, although it may take years to be sure.

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RESPONSE

In their wide-ranging critique, Helmut Norpoth and Jeffrey Segal raise both theoretical and methodological objections. They assert not only that we fail to justify the direct effects observed between public opinion and Supreme Court decisions but also that there are good theoretical and empirical reasons to doubt such effects. They also conduct "a methodological audit" of our analyses, alleging that we (1) use "aggregate-level analysis on individual-level theory," (2) specify an atheoretical lag structure of dubious empirical validity, and (3) fail to recognize or control serious problems of mean-nonstationarity in our measure of Court decisions.

In response, we argue first that theory and research on the impact of public opinion on Supreme Court decisions are both in short supply. None of the hypotheses in the literature, including that which they prefer, is derived from well-developed theory. Yet all are sufficiently plausible to warrant empirical scrutiny. Second, the debate over the impact of public opinion on Supreme Court decisions raises both aggregate- and individual-level questions, but the aggregate questions are logically prior and more important. Moreover, contrary to Norpoth and Segal's claim, data on individual-level effects are wholly consistent with our aggregate conclusions. Finally, we defend the lags in our models, demonstrate that nonstationarity is not a problem (in part because the principal variables are cointegrated), and argue that Norpoth and Segal misspecify our model. A properly specified model, estimated appropriately, confirms that public opinion has both direct and indirect effects on Supreme Court decisions during the Warren and Burger years.

THEORIES OF POPULAR INFLUENCE ON SUPREME COURT DECISIONS

Norpoth and Segal grant our larger point that the Supreme Court is responsive to public opinion. The controversy centers on whether the effects of public opinion are wholly mediated though the impact of presidential appointments on the ideological composition of the Supreme Court or whether public opinion can influence Court decisions more directly. The most perplexing criticism, they advance, is that we fail to justify the hypothesized direct effects of public opinion. In fact, we devoted more than a page to explaining possible effects, most of which discussion focuses on the rationale for direct effects (1993b, 88–90). Importantly, the direct-effects hypothesis is *not* original to us. It has a long and distinguished pedigree, as we documented. Our contribution is simply to test the hypothesis empirically.

The direct-effects hypothesis has two distinct explanations. One, linked to Justice Frankfurter, holds that the Court is a political institution whose authority depends on public deference and respect. Sensitive to this, justices are careful not to jeopardize the Court's authority by departing too far or too long from majoritarian views on fundamental issues. The theory is not that justices routinely change their decisions based on public opinion polls but that *some* justices *occasionally* modify their decisions (if not their personal beliefs) on important *issues* in response to *long-term and fundamental* changes in public opinion perceived as threatening the Court's authority. A second explanation, which we link to Justice Rehnquist, holds that justices are influenced (albeit often unconsciously) by the same forces that impinge society as a whole. Here, the argument is not that justices vote contrary to their beliefs in response to public sentiment but that the beliefs of at least *some* justices *occasionally* change in response to *fundamental long-term shifts* in what Stimson calls the public mood, and Lippman, the public philosophy. In retrospect, we probably erred in applying the label, "political adjustment hypothesis" to both versions. The Rehnquist perspective might better be labeled a "political socialization" or "attitude change" hypothesis. Both perspectives, however, predict that public opinion will influence Court decisions unmediated by presidential election/selection.

Norpoth and Segal go further, however, and assert that the direct-effects hypothesis is positively insupportable in theory and contradicted by existing research. They reason that since justices are not electorally accountable, they will respond to public opinion only if they "normatively feel that they ought to." Moreover, if justices respond to public opinion out of a sense of civic-mindedness. Norpoth and Segal claim that they certainly would advertise this in their decisions, which, apparently, they do not.

This, however, is a straw man. First, the hypothesis that direct effects can occur only if justices hold democratic role orientations is not one we advanced or discussed. Second, the supportive quotations cited from Justices Jackson, Scalia, and Marshall are normative statements to the effect that public opinion *should not* influence Court decisions. They do not address the empirical question whether public opinion *does* influence the Court or speak to the plausibility of either the political adjustment or socialization hypotheses.[2] Third, the observation that justices rarely cite public opinion as justification for their decisions is irrelevant. If justices respond to public opinion via the political adjustment or socialization pathways, they are unlikely to advertise the fact that they are "caving in" to public pressure or have changed their minds in response to evolving social mores. Consider, for example, Segal and Spaeth's (1993) argument to the effect that judicial decisions are driven principally by the attitudes and beliefs of individual justices. If this is true (and we are generally persuaded), in what percentage of cases does the majority defend its decision in terms of the justices' personal attitudes and beliefs? We doubt the figure is as high as 2%. The point, of course, is that such evidence is meaningless.

Norpoth and Segal also cite, without discussion, two articles that, they claim provide evidence contradicting public opinion effects (Giles and Walker 1975; Songer, Segal, and Cameron 1994). In fact, neither article addresses the issue in any substantial manner. Giles and Walker report a nonsignificant, zero-order correlation between an indirect measure of federal district court desegregation policies in 1970 and the percentage of votes cast in judicial districts for George Wallace in 1968—hardly a stern test of public opinion effects on Supreme Court decisions, nor do Giles and Walker claim otherwise. Songer, Segal, and Cameron examine whether individuals on the U.S. Courts of Appeals decide search-and-seizure cases according to personal policy preferences or the policy dictates of the Supreme Court. Public opinion is not addressed except in a single note claiming that the inclusion of Stimson's public mood variable in a multivariate model produced an insignificant coefficient. The results are not shown, and no mention is made as to whether the specification for public opinion was appropriately lagged, included an interactive term for public opinion during the Reagan years, or attended to other important methodological and specification issues. Since this is the totality of research cited in defense of the proposition that public opinion effects are contradicted by previous research, we think it fair to conclude that our decision to explore public opinion effects in a more systematic and rigorous fashion is reasonable and appropriate.

More generally, the attack on the theoretical plausibility of public opinion effects seems misplaced. What passes for theory in this area on both sides of the issue is little more than speculation mixed with substantial doses of wishful thinking and post-hoc rationalization. Norpoth and Segal have their preferred hypothesis. Other scholars and jurists suggest several others. None is deduced from first principles or is supported by much systematic research—which is why we chose to examine the issue more rigorously.

ON THE THEORY AND EVIDENCE OF A FIVE-YEAR LAG

Norpoth and Segal also take us to task for failing to justify the five-year lag specified for the impact of public opinion. They acknowledge our prediction that the political adjustment and socialization effects should occur at significant lags, but they criticize our failure to provide a "precise" theoretical prediction of the lag's length. This seems unreasonable. Existing theory does not—and cannot reasonably—provide a precise specification of this lag any more than the Segal–Spaeth attitudinal model predicts the precise impact of judicial attitudes on Court decisions. Segal and Spaeth estimate empirically the impact of attitudes on Court decisions just as we use empirical methods to estimate lag-lengths. Consider a group of men during the 1960s and 1970s who began to comprehend the reality of gender discrimination when it was experienced by their wives and daughters seeking educational and employment opportunities. Precisely how long does theory predict it will take the average male to begin to reexamine and change long-held attitudes and values about the role of women? No doubt it varies across individuals. We know of no theory that specifies the precise lag or even a rough estimate. Even if such theory existed it likely would be of little help in identifying the lag specifically for the small and atypical group of Supreme Court justices. Rather than resort to post hoc rationalization, we think it preferable to admit the limits of theory and proceed to specify the lag-lengths empirically.

Norpoth and Segal express doubts about these empirical procedures, too. They argue that by searching across lags up to 10 years we had a 43% chance of "falsely finding" a significant lag purely by chance. First, the 43% figure holds only if the 10 correlations represent independent observations; they do not, since all are drawn from the same interdependent time series. Second, as Norpoth argues in his aggregate time-series research on British economics and elections, statistical tests of significance are not the acid test of the importance of a relationship when dealing with a historical population and very short time series. The small number of observations makes finding any significant relationships extremely difficult. At least as important as the significance of the correlation at the five-year lag is the fact that the size of this correlation (.40) is almost *three times larger* than the correlation at any other lag (see Mishler and Sheehan 1993b, tbl. 1). Third, because the public opinion and Court decision variables are both filtered by a powerful first-order autoregressive process (AR = .73), the probability of finding any significant correlation is severely circumscribed (see Norpoth 1986; Norpoth 1992, 141–42; Norpoth and Yantek 1983). When the two series are analyzed without prewhitening, as Norpoth and Segal suggest, all of the correlations are statistically significant at lags 0–5, though lag 5 is still the strongest.[3] Finally, examination of cross correlations is only a first step in identifying appropriate lags. If a lagged correlation is falsely significant, the relationship should prove spurious when examined in a fully specified model. The five-year lag, however, remains statistically significant and robust when included in a properly specified multivariate model.

Finally, Norpoth and Segal ask why, if justices are influenced by public opinion at a lag of five years, there is no evidence of effects at earlier lags. This is a reasonable question and one we should have addressed. Logically, both the political-adjustment and attitude-change hypotheses predict that public opinion will have a gradual, permanent impact on the Court's decisions. The impact should be small at the moment but should gradually increase over time before ending or leveling off at some impossible-to-predict future point. Nevertheless, we modeled the relationships as an abrupt–permanent effect at five years. We did so not because we were indifferent to the shape of the effect but because we were concerned with preserving precious degrees of freedom given a relatively complex model and a very small number of cases.

TABLE 5

Distributed Lag Estimates of Public Mood Effects on Supreme Court Decisions, 1956–1989

COEFFICIENT	ESTIMATE	S.E.
Mood_t	−.12*	(.06)
Mood_{t-1}	−.06	(.05)
Mood_{t-2}	−.01	(.05)
Mood_{t-3}	.06	(.05)
Mood_{t-4}	.12	(.07)
Mood_{t-5}	.18*	(.08)
Mood_{t-6}	.24*	(.10)
Mood_{t-7}	.31*	(.12)

Note: Standard errors are given in parentheses. Lag estimates calculated from the coefficients for the first-order polynomial specification for public mood in the following Almon distributed-lag equation:

$$D = a_t + b_1C_{t-1} + b_2R_t + b_3(R \times M)_{t-5} + b_4M_t ** 0 + b_5M_t ** 1 + u_t,$$

where D = liberalism of court decisions, C = liberalism of court composition, R = intercept for Reagan years, $R \times M$ = interactive term for public opinion in Reagan years, and M = public mood.
*$p \leq .05$ (one-tailed test).

To determine the consequences of this decision, we have reestimated the impact of public opinion on the Court's decisions as a first-order polynomial in an Almon distributed-lag model.[4] In addition to the specification for public mood, the model includes controls for Court ideology[5] and the effects of public opinion during the Reagan years. The results, reported in Table 5, support the hypothesis of gradual effects albeit over about seven years rather than five. The results indicate that the instantaneous effect of public opinion on the Court's decisions is slightly though significantly negative. Rather than indicating that justices react perversely to changes in public opinion, the negative coefficient is better interpreted as a measure of the Court's inertia—a reflection of the time it takes justices to perceive and respond to a significant change in the public mood. The impact of public mood turns positive by year 3 and grows steadily larger through year 7. Importantly, year 5 is the first in which the positive lagged effects are statistically significant. Models (not shown) specifying maximum lag-lengths greater than seven years show insignificant effects for years beyond seven. Models specifying less than a seven-year effect have significant lagged coefficients similar to the seven-year model but a weaker fit (i.e., smaller R-squareds).

Clearly the five-year lag is a simplification of reality, but it seems a reasonable compromise between theoretical complexity and methodological necessity.

INDIVIDUAL VERSUS AGGREGATE ANALYSIS

Norpoth and Segal further charge that we perform "aggregate-level analysis on individual-level theory." It is true that individual-level theory underlies some of the aggregate-level hypotheses. We always intended to conduct additional research on individual dynamics and have subsequently done so (Mishler and Sheehan 1993a). However, these micro concerns are of secondary importance. The more important questions concern macro processes or cut across levels of analysis such that the appropriate level is at least debatable (see, e.g. Erbring 1989; Kramer 1983). As we made clear our interest is not to test theories of individual decision making but to answer a series of questions about the role of the Court in democratic theory, specifically, (1) Are majority (i.e., aggregate) decisions of the Supreme Court broadly consistent with majority rule (i.e., aggregate public opinion)? and (2) Is the Court's aggregate responsiveness to majority rule mediated exclusively through the aggregate-level process of presidential election and judicial selection, or can it occur through other means as well? The important question for democratic theory is not whether individual justices respond to public opinion but whether decisions by the majority of the Court are countermajoritarian. Moreover, one need not reject methodological individualism to recognize that group outcomes often are not definable simply as sums of individual behaviors. Composition rules (i.e., how individuals interact in groups and how behavior is aggregated) also are important and can produce aggregate results quite at odds with individual actions (Brodbeck 1968).

Even conceding that individual dynamics are an important, subsidiary issue, we find nothing in the individual data reported by Norpoth and Segal to contradict our aggregate results. Although most of the 30 correlations in Table 3 between public mood and the decisions of five justices in one policy area are not statistically significant, *every one* of the correlations has a positive sign. If there really is no relationship between public opinion and individual decisions, we would expect about half of the correlations to be negative purely by chance. Assuming (erroneously, of course) that these are independent observations, the probability of obtaining 30 consecutive positive correlations from a population where the true correlation is zero is approximately $\frac{1}{2}^{30}$ or slightly less than one in a billion! This seems a reasonable indication that variations in the decisions of at least some Supreme Court justices are related to fluctuations in the public mood. Indeed, our subsequent research and related work by Wood and Flemming (1993) confirm that this is the case. Analyses of the 15 longest serving justices on the Court between 1953 and 1992 indicate that the decisions of as many as a third of the justices are significantly related to fluctuations in public opinion at various lags (Mishler and Sheehan 1993a).

Of course, it is not necessary for all members of the Court to respond to public opinion with equal sensitivity, at the same speed, or even at all. A single member, who is significantly affected by public opinion, such as Justice White, can alter majority decisions in cases where the Court is closely divided. Interestingly, the evidence in Table 3 that Justice White is significantly influenced by public opinion has considerable face validity. It accords with his reputation for having changed his personal views on a variety of issues during his tenure on the Court (see, e.g., Segal and Spaeth 1989). That he appears to respond to public opinion at a lag of four to five years, that this change occurred mostly during the late 1960s and early 1970s (not shown), and that his opinions become increasingly conservative with time all correspond remarkably well with our aggregate results.

Norpoth and Segal want to dismiss the evidence of Justice White's responsiveness to public opinion because most of the observed correlation disappears when the dramatic changes in public opinion that occurred in the mid-1960s are controlled. However, by discarding this most dramatic change in public mood, you effectively ignore the only substantial and sustained shift in public mood that occurs before the 1980s. Understandably, if you remove the principal source of variation from a short time series, the residuals are unlikely to correlate with much of anything. Given that Justice White's decisions become increasingly conservative over time, it is hardly surprising that eliminating the conservative mood swing in the 1960s significantly reduces the correlation between public opinion and his decisions. In effect, Norpoth and Segal demonstrate that when big shifts in public opinion are ignored, there is little evidence that the Court or its members are influenced by the small and seemingly random fluctuations in public opinion that remain. But then our argument always has been that the Court is most likely responding to substantial and sustained changes in the public mood. Far from contradicting our arguments, the individual-level data reported by Norpoth and Segal complement our aggregate-level results.

NONSTATIONARITY

A final criticism of our methods is that we violate the assumption of mean nonstationarity in analyzing the liberalism of Court decisions. Standard diagnostics confirm that the liberalism of Court decisions is nonstationary. The usual way to handle this is to difference or prewhiten the variable before analysis and then to apply the same transformation to the independent variables. Norpoth cautions, however, "these transformations risk 'throwing the baby out with the bath water' . . . and may yield positive results only under rare circumstances" (1992, 141–42). The technical problem is that the transformed series may be "overdifferenced" and important information lost especially regarding long-term effects.

Mindful of this, political scientists usually assume that nonstationarity in the dependent variable poses few problems as long as its residuals are free of autocorrelation after a properly specified analytic model is applied (Norpoth 1986; Norpoth 1992; Norpoth and Yantek 1983). This is the approach Norpoth and Segal use in their reanalysis of our data. They do not difference or detrend Court decisions prior to analysis but analyze the data as if it were stationary, apparently because the residuals are white noise. This is precisely the same approach we followed. Thus, according to the identical criteria Norpoth and Segal apply to their models, nonstationarity is not a problem in our original analyses either. The standard time-series diagnostics we reported indicate that the residuals of all of the models are white noise. In every case, the Durbin–Watson and Ljung–Box Q statistics are nonsignificant indicating that the residuals are appropriately free from autocorrelation (see Mishler and Sheehan 1993b, tbls. 2–3). Importantly, as well, inspection of the residual autocorrelation functions for our models (not shown) indicates that there are no significant residual correlations in any of the models at any lags up to 12 years. Thus nonstationarity does not distinguish our models and theirs.

There is, however, a different problem both with our models and with the alternatives proposed by Norpoth and Segal that we previously overlooked. The problem stems from the fact that the major independent variables, public mood and Court composition, are also nonstationary.[6] Recent work in econometrics holds that if time series with similar levels of nonstationarity are regressed on one another and produce stationary residuals, then the series are cointegrated (Engle and Granger 1987). It now appears that the relationships between Court decisions and both Court composition and public opinion are cointegrated.[7] This is true both for our models and those estimated by Norpoth and Segal. Although ordinary least squares regressions of cointegrated series produce consistent estimators, these estimates are not fully efficient, and the distributions of the estimators (standard errors) are generally nonnormal. Thus inferences cannot be drawn regarding statistical significance.

Engle and Yoo (1989) propose à three-step procedure for adjusting ordinary least squares estimates and producing appropriate standard errors for cointegrated series.[8] Table 6 reports both the original and the corrected estimates for the basic model (originally reported in Mishler and Sheehan 1993b, tbl. 2-B). In these models, the Courts' decisions are regressed against public mood (modeled for simplicity with a lag of five), Court composition (with a lag of one), and an interactive term for public mood during the Reagan years. The corrected results are very similar to the original. The principal differences are that the corrected effects of public mood are somewhat larger and those of Court composition are somewhat smaller than originally reported. Nevertheless, both public mood and court composition have significant effects (.10, level) on the Courts' decisions at the specified lags.[9] Of course, this strengthens our conclusions. Reestimations (not shown) of the other models in our original analysis also produce results very close to the original.

TABLE 6

Original and Error-corrected Regressions of Supreme Court Decisions, 1956–89

INDEPENDENT VARIABLES	ORIGINAL ESTIMATES	ERROR-CORRECTED ESTIMATES
Intercept	−4.87 (25.57)	−29.95 (19.20)
Public mood$_{t-5}$[a]	.52 (.24)	.94* (.32)
Court composition$_{t-1}$[b]	2.73 (.79)	1.47* (.80)
Reagan/mood$_{t-5}$	−.97 (.55)	−.32 (1.10)
Reagan intercept[c]	93.24 (59.04)	40.95 (96.89)
AR(1)	−.52 (.18)	−.01 (.21)
R^2(reg)[d]	.71	—
R^2(tot)[e]	.78	—
Durbin–Watson	1.92 (ns)	— —
Ljung–Box Q (df = 12)	5.42 (ns)	—

Note: Estimates are maximum likelihood estimates. Standard errors are in parentheses and are consistent only for error-corrected estimates. N = 34.

[a] Index of the liberalism of public opinion on domestic policy concerns.

[b] Sum of liberalism scores of Supreme Court justices at time of initial appointments.

[c] Dummy varible coded 1 for years 1981–89, 0 otherwise.

[d] Variance explained by model excluding autoregressive parameter.

[e] Variance explained by model including autoregressive parameter.

*$p \le .05$ (one-tailed test).

**$p \le .01$ (one-tailed test).

In sum, the alternative analyses reported by Norpoth and Segal have exactly the same statistical properties as those reported originally. All are stationary (because they are cointegrated, not because of the inclusion of dummy variables) and when appropriate error corrections are applied, all produce results consistent with those initially reported.

SPECIFICATION ISSUES

What, then, do we make of the fact that the inclusion of the dummy variable by Norpoth and Segal significantly attenuates the coefficient for the public mood and causes it to become statistically nonsignificant?

What Norpoth and Segal do not show in their analysis is that inclusion of the same dummy variable in their model (Table 4) causes both public mood and Court composition to become nonsignificant. The same results obtain when the dummy variable is included in a model with Court composition as the only independent variable.[10] This is true, moreover, whether Court composition is lagged one year or not. Just as the dummy variable makes it appear that the effect of public mood on Court decisions is spurious, it has exactly the same effect on the impact of Court composition—suggesting, nonsensically, that neither Court composition or public mood influence the Court's decisions. The problem, again, is that by removing the principal source of variation from a short time series, it is almost inevitable that the series will fail to correlate with anything.

The problem with dummy variables, as Hanushek and Jackson point out, is that their use "admits to a lack of knowledge and/or data" (1977, 103). The dummy variable in these analyses indicates only that "something happened" in the late 1960s and early 1970s with respect to public opinion, the composition of the Court, and the Court's decisions. The dummy variable cannot tell us what happened, in what order, with what causes, or with what direct, indirect, or intervening consequences. By labeling the dummy variable "Court realignment," Norpoth and Segal arbitrarily designate membership change as the principal dynamic during this period and credit it for virtually all of the change in Court decisions before and after during this critical period. This, however, is entirely speculative; it is not based on any data regarding public opinion or Court composition. Had they chosen to call the dummy "public opinion change," then public opinion would be credited as the overriding determinant of changes in the Court's decisions. In the absence of other information, both interpretations/speculations are equally justified. Indeed, given the evidence that the conservative tide in the Court's decisions began *before* the conservative shift in the Court's composition but *after* the rightward change in public opinion had begun (Mishler and Sheehan 1993b, figs. 1–2), the logic of causal inference would seem to place the burden of proof on those who contend that the latter-occurring change in the Court's composition was the cause of the earlier-occurring conservative trend in Court decisions.

More importantly, there is no need to engage in such ad hoc speculation about the meaning of a dummy variable. In this case, we have direct measures of both public mood and Court composition with which to estimate empirically their relative effects on Court decisions. Indeed, one of our main purposes was precisely to avoid such arbitrary judgments and to assess empirically the strengths of different pathways by which public opinion might influence the Court. A dummy variable cannot do this. We believe we have demonstrated that when measured directly, both public opinion and Court composition[11] can be shown to have significant and independent effects on Court decisions. Although the effect of Court composition probably has somewhat stronger direct effects (a point we always have acknowledged), public opinion has significant independent effects as well.

Why, then, do Norpoth and Segal find, when they replicate our model without the dummy variable, that Court composition is significant but public mood is not (Table 4)? The answer is that they do not replicate our model. Our model includes a multiplicative term for the effects of public opinion during the Reagan years, whereas theirs does not. Because public opinion has little or no effect on Court decisions during the Reagan years, the failure to distinguish the two periods results in an underestimation of the effects of public opinion during the pre-1980 period. The only difference in our two models is that they treat the impact of public opinion as constant across the entire period, whereas we allow the possibility that public opinion has a different impact during the Reagan years.

But why should this be the case? Why should the Reagan years be different? And if they are, why was this not also the case for the entire Burger–Rehnquist period? To elaborate, briefly, an argument offered in our conclusion, we believe the answer lies in the way that the Court aggregates individual preferences into collective decisions. The Court's majority-rule criteria means that the public opinion can influence majority decisions only if the impact of public opinion on individual justices is widespread or if the ideological distribution of opinions among justices is roughly balanced. Since we assume that judicial attitudes are relatively resistant to change, it is unlikely that public opinion will have widespread individual effects. Thus collective effects are likely to depend on the existence of ideological balance. As long as the Court is reasonably balanced, the impact of public opinion on even a single justice may be sufficient to alter the Court's decisions at the margins. But when the ideology of the Court becomes such that a change in the opinion of one or two justices is insufficient to alter majority outcomes, then public opinion is likely to have little direct influence on the Court's decisions (at least, so we continue to speculate.[12] In effect, President Reagan's appointments of Justices O'Connor, Scalia, and Kennedy turned a moderately conservative Court into a highly conservative one, thus insulating the Court from direct public opinion effects.

Finally, implicit in Norpoth and Segal's critique is an assumption that the direct-effects hypothesis contradicts the attitudinal model of judicial decision making defended most recently by Segal and Spaeth (1993). According to this model, judges make decisions "by considering the facts of the case in light of their ideological attitudes and values" (p. 72). We believe, in contrast, that the direct-effects hypothesis is fully consistent with an attitudinal model. Except in the rare cases where a justice votes contrary to personal beliefs to preserve the authority of the Court in the face of public opposition, our view is that the impact of public opinion is likely to be mediated entirely through the attitudes and ideologies of the justices. This is obvious in situations where the public elects a new president who selects like-minded justices to fill judicial vacancies, thereby altering the ideological balance of the Court. Although less obvious, this also is the case for the political socialization process where public opinion alters the Court's ideology by causing one or more incumbent justices to change their individual attitudes. Rather than viewing aggregate ideological change as a consequence of attitudinal *replacement*, the political socialization hypothesis attributes aggregate ideological change to attitudinal *conversion*.

Our analysis, then, does not argue against the attitudinal model of judicial decision making but in favor of a more subtle version in which individual attitudes are treated not as static and immutable but as fluid and dynamic. We do not doubt that judicial attitudes are highly viscous, especially compared to the political attitudes of individual citizens. What we question is the summary dismissal of the possibility of change. Thus, while the impact of public opinion on Court decisions is inconsistent with a naive attitudinal model that assumes the impossibility of individual change, it is fully consistent with a more sophisticated version that views individual change as one of the important dynamics underlying aggregate changes in Supreme Court decisions.

SUMMARY

It should be obvious that our understanding of the relationship between public opinion and Supreme Court decisions remains rudimentary. There is much to learn. Norpoth and Segal are correct in identifying individual-level effects as one area where additional work is needed. If, as we suspect, public opinion has widely varied effects on different individuals, then it would be interesting to know which justices have responded most (or at all) to these effects. Are moderates more responsive to opinion change than ideologues? Are younger or newer justices more responsive than older ones? Do liberal justices respond more to liberal mood shifts and vice versa? Under what conditions are individual effects most likely? Aggregate-level questions abound as well: Are we correct in our speculation that the ideological balance of the Court is critical for public opinion effects? What other conditions may constrain or enhance this relationship? Are the effects of public opinion symmetrical or asymmetrical? Is there a critical threshold of opinion intensity or duration necessary for public opinion to have an impact? Does responsiveness to public opinion vary by issue domain? And (of course) is the responsiveness of the Court during the Warren and early Burger years typical or an aberration? Much work remains. We appreciate Norpoth and Segal's attention to our work. Although we believe their criticisms mostly miss the mark, we look forward to continued fruitful exchanges.

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Notes

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1. One might also quote Felix Frankfurter, who apparently was of two minds on this subject: "The Court has no reason for existence if it merely reflects the pressures of the day" dissenting; (*West Virginia Board of Education* v. *Barnette* (1943, 665).

2. It is also possible that the justices knowingly react to public opinion but feel that they ought not to and thus do not advertise it in their opinions. This is an explanation that neither rational choice models nor role theory models can account for.

3. Cook (1977) shows a high correlation between public opinion about the Vietnam War and sentencing in draft cases, but the claim of influence is effectively negated by Kritzer (1979).

4. See Mishler and Sheehan 1993b, 15. The situation would not be as severe if there were a priori predictions about particular lags. For instance, in quarterly data, lags at multiples of four could be expected, and in election results, lags of two, four, or eight years might be expected.

5. Data for the justices were generated from the U.S. Supreme Court Judicial Database, using citation plus split votes as the unit of analysis.

6. These correlations were not prewhitened, since the decision of the individual justices, except for Justice White, displayed no significant autocorrelations.

7. We further note that the Segal and Cover data, designed to predict civil liberties decisions, may not be a good measure of the values of the justices in economic and union cases, which make up a substantial proportion of the Mishler–Sheehan data base. As demonstrated by various scholars, economic decisions and civil liberties decisions are distinct dimensions (e.g., Rohde and Spaeth 1976; Schubert 1965). Indeed, the r-squareds between Court ideology, (as compiled using the Segal–Cover scores) and the Court's annual economic/union decisions are only .03 and .02, respectively. Thus the Segal and Cover scores may not sufficiently control for ideological change on the Court for the type of data used by Mishler and Sheehan, particularly at a one-year lag.

8. We cited Justices Rehnquist and Frankfurter to the effect that public opinion *can and does* influence the Court. No doubt, Norpoth and Segal could find additional skeptical quotations, just as we could find additional supportive ones. We will refrain, however, since we have little confidence in the quotation/counterquotation method of social-scientific inquiry. Ironically, on the day we received Norpoth and Segal's critique, a colleague, commenting on the article, claimed that he was not surprised by our conclusions, since all we had really done was to demonstrate statistically what everyone already knew!

9. The simple correlation between the unfiltered and unlagged measures of public mood and Court decisions is .52. The correlation increases steadily for lagged values of public mood reaching a maximum correlation of .68 at a lag of five years. The correlation decreases for lags in public mood greater than five years.

10. Almon distributed-lag models assume that the coeffi-

cients for successive lags of the independent variable can be approximated by a suitable degree polynomial. The approach uses ordinary least squares or maximum likelihood estimates procedures to estimate the coefficients for the polynomial, using variables constructed from lags of the original independent variables and then calculates the specified lags of the independent variables from the weighted polynomial coefficients. Unfortunately, both the order or the polynomial and the length of the lag need to be specified apriori. The hypothesis that public opinion has a gradual permanent effect on Court decisions suggests either a first-order (i.e., linear increase in effects) or second-order (asymptotic increase in effects) polynomial. We estimated both and found that whereas the first-order polynomial had significant coefficients and a high R-squared, the coefficient for the second-order polynomial was nonsignificant. For a good discussion of distributed-lag models, see Gujarati (1988, 534–41).

11. Norpoth and Segal also question the one-year lag for the Court compositions variable. Two considerations underlie this lag. First, because most new justices during this period joined the Court in midterm and participated, on average, in fewer than a third of the cases, it seemed logical to assume that the full impact of most new appointments would not be realized until their second year (i.e., at a lag of 1). Second, the prewhitened cross correlation between Court composition and decisions was more than half again as large at a lag of 1 as at a lag of 0 (r = .44 versus .28; see Mishler and Sheehan 1993b, tbl. 1). Indeed, the unlagged correlation is not statistically significant. Thus a one-year lag makes sense on both theoretical and statistical grounds. In practice, however, substituting an unlagged ideology term for the one-year lag used in our original models has no appreciable effect.

12. Dickey–Fuller tests indicate that the hypothesis of nonstationarity cannot be rejected for any of the three series. The Dickey–Fuller results were as follows:

Time Series	Parameter	*t*-ratio
Court decisions	−.21	−1.87
Public mood	−.15	−1.53
Court composition	−.03	−.57

The critical *t*-ratio (.01 level) for N = 25 is −3.75. A ratio less than (i.e., a larger negative than) this threshold is necessary to reject the null hypothesis of nonstationarity. Thus in all of these series the hypothesis of nonstationarity must be accepted. Augmented Dickey–Fuller tests for lags up to five years produce identical results for all three series.

13. The cointegration regression of Court decisions on public mood$_{t-5}$ and Court composition$_{t-1}$ produced residuals with a Durbin–Watson *d* of 1.74. The coefficient for the first-order lagged residuals is −.87 with a Dickey–Fuller *t* of −4.74 and an augmented Dickey–Fuller *t* (for five lags) of −3.87. Given critical values (.05 level) for these three statistics of .78, −3.67, and −4.15, respectively, the null hypothesis on nonstationarity must be rejected. Similar results are obtained when either public mood or Court composition is regressed on Court decisions independently or when both are included in a cointegrating equation with controls for public mood during the Reagan years.

14. The three steps involve (1) estimation of a standard regression of the cointegrated series: $Y_t = \alpha X_t + Z_t$, where Z is the ordinary least squares residual; (2) estimation of a second-step dynamic equation using the residuals from the first stage to impose a long-run constraint: $\Delta Y_t = \Phi(L)\Delta Y_{t-1} + \Omega(L)\Delta X_T + \delta Z_{t-1} + u_t$; and (3) estimation of a third-step equation consisting of a further regression of the independent variables from the first-stage equation multiplied by minus the error correction parameter from equation 1, regressed against the residuals of the second-step equation: $u_t = \varepsilon(-\delta X_t) + v_t$. The third step provides a correction to the parameter estimates of the first-step, static equation which makes them asymptotically equivalent to *full information maximum likelihood* and provides a set of standard errors that permits the calculation of standard *t*-tests. The correction for the first-step estimates is then simply $\alpha^3 = \alpha^1 + \varepsilon$, and the correct standard errors for the adjusted step 1 estimates are those given by the step 3 regression. In effect, the second-step equation provides an estimate of short-term effects, the third-step equation measures long-term dynamics, and the sum of the two provides an estimate of total effects.

15. Although the corrected coefficient for public mood during the Reagan years remains negative, it is no longer statistically significant. In other words, the increasing liberalism of public opinion had little or no impact on the Court's decisions during the Reagan years. However, it does not have the perverse effect, implied in the original analysis, of causing the Court to become even more conservative. Of course, the number of observations post 1980 are so small that the interactive term for the Reagan years should be interpreted with caution. Its primary function is as a statistical control.

16. For models including public mood and the realignment dummy, the coefficient for Court composition is .81 with a standard error of .83. In the model including only Court composition and the dummy realignment variable, the coefficient and standard error for Court composition are 1.01 and .62, respectively.

17. Norpoth and Segal also question our use of the Segal and Cover (1989) measure of the ideology of Supreme Court justices. They note that this measure is based on the justices' prenomination positions only on civil liberty issues and may not reflect judicial attitudes on other issues (esp. economic issues). Unfortunately, the Segal–Cover measure is the only measure of Supreme Court ideologies, with which we are familiar, which is not based on the actual decisions of justices. Although relatively more abundant, measures of the latter type suffer the disadvantage of being circular: ideology is measured based on a justice's decisions and then used to predict that same justice's decisions. Although imperfect, the Segal–Cover measure is the best available and is superior to the dummy-variable alternative, which assumes that all of the change in the Court's decisions before and after 1970 are consequences of unmeasured ideological change. Importantly, the prewhitened correlations between the Court composition index and separate measures of the liberalism of (1) all Court decisions (i.e., the measure used in our analysis and by Norpoth and Spaeth), (2) Court decisions in the subset of criminal procedure cases, and (3) Court decisions in the subset of race-related civil rights cases are remarkably similar, especially given the small number of cases and the different detrending filters used in each case. The correlations for the unlagged Court composition measure are .28, .40, and .34, respectively. The correlations when Court composition is lagged one year are .44, .47, and .23, respectively. This provides a measure of reassurance that the ideology measure has broad applicability.

18. The logic of this argument implies that what is important about ideological balance is the distribution of justices on either side of an issue and not the relative intensity or distance from the cutting point of their ideological preferences. Thus the observation that the net liberalism of Supreme Court justices was larger in the early 1960s than their net conservatism in the 1980s is less important to this argument than the relatively more balanced sizes of the liberal, conservative, and moderate voting blocs in the 1960s compared to the 1980s.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN DECEMBER, 1994

The following articles, research notes and controversies have been tentatively scheduled for publication in a forthcoming issue.

James E. Alt and Robert C. Lowry. "Divided Government and Budget Deficits: Evidence from the States."

Stephen Ansolabehere, Shanto Iyengar, Adam Simon, and Nicholas Valentino. "Does Attack Advertising Demobilize the Electorate?"

Mary G. Dietz. "'The Slow Boring of Hard Boards': Methodical Thinking and the Work of Politics."

Jorge I. Dominguez and James McCann. "Shaping Mexico's Electoral Arena: The Construction of Partisan Cleavages in the 1988 and 1991 National Elections."

James H. Lebovic. "Riding Waves or Making Waves? The Services and the U.S. Defense Budget, 1981–1993."

Steven C. Poe and C. Neal Tate. "Repression of Human Rights to Personal Integrity in the 1980s: A Global Analysis."

John T. Scott and Vickie B. Sullivan. "Parricide and the Plot of *The Prince*: Cesare Borgia and Machiavelli's Italy."

Stergios Skaperdas and Bernard Grofman. "Modeling Negative Campaigning."

Lonna Rae Atkeson and Randall W. Partin. "Economic and Referendum Voting: A Comparison of Gubernatorial and Senate Elections." A research note.

Ross E. Burkhart and Michael S. Lewis-Beck. "Comparative Democracy: The Economic Development Thesis." A research note.

James C. Clingermayer and B. Dan Wood. "Disentangling Patterns of State Debt Financing." A research note.

Kevin B. Grier, Michael C. Munger, and Brian E. Roberts. "The Determinants of Industry Political Activity, 1978–1986."A research note.

Robert Rohrschneider. "Report from the Laboratory: The Influence of Institutions on Political Elites." A research note.

George F. Bishop, Alfred J. Tuchfarber, and Andrew E. Smith; Paul R. Abramson and Charles W. Ostrom. "Question Form and Context Effects in the Measurement of Partisanship: An Experimental Test of the Artifact Hypothesis." A controversy.

BOOK REVIEWS

American Political Science Review

Book Review Contents

POLITICAL THEORY

Botwinick, Aryeh. *Postmodernism and Democratic Theory* .... Nancy S. Love 732
Chapman, John W., and Ian Shapiro, eds. *Democratic Community: NOMOS XXXV* .... William A. Galston 732
Gaskins, Richard H. *Burdens of Proof in Modern Discourse* .... Gavan Duffy 733
George, Robert P. *Making Men Moral: Civil Liberties and Public Morality* .... Richard C. Sinopoli 734
Gunnell, John G. *The Descent of Political Theory: The Genealogy of an American Vocation* .... J. Donald Moon 735
Hirsch, H. N. *A Theory of Liberty: The Constitution and Minorities* .... Mark Tunick 736
Jones, John Paul III, et al., eds. *Postmodern Contentions: Epochs, Politics, Space*
Poster, Mark, ed. *Politics, Theory, and Contemporary Culture* .... Mark E. Warren 738
Kekes, John. *The Morality of Pluralism* .... Steven Lukes 739
Marcus, George E., and Russell L. Hanson, eds. *Reconsidering the Democratic Public* .... Peter Bachrach 740
Masters, Roger D. *Beyond Relativism: Science and Human Values* .... Tatu Vanhanen 741
Minowitz, Peter. *Profits, Priests, and Princes: Adam Smith's Emancipation of Economics from Politics and Religion* .... Donald Winch 742
Mitchell, Joshua. *Not By Reason Alone: Religion, History, and Identity in Early Modern Political Thought* .... A. P. Martinich 743
Murphy, James Bernard. *The Moral Economy of Labor: Aristotelian Themes in Economic Theory* .... Judith A. Swanson 744
Nozick, Robert. *The Nature of Rationality*
Steinberger, Peter J. *The Concept of Political Judgment* .... Howard Margolis 745
Oakeshott, Michael. *Morality and Politics in Modern Europe: The Harvard Lectures*
Oakeshott, Michael. *Religion, Politics and the Moral Life* .... Patrick Riley 746
Rescher, Nicholas. *Pluralism: Against the Demand for Consensus* .... Anthony Arblaster 748
Sinopoli, Richard C. *The Foundations of American Citizenship: Liberalism, the Constitution, and Civic Virtue* .... Forrest McDonald 749
Wolfenstein, Eugene Victor. *Psychoanalytic-Marxism: Groundwork* .... Ernesto Laclau 750

AMERICAN POLITICS

Barrett, David M. *Uncertain Warriors: Lyndon Johnson and His Vietnam Advisers*
Wood, Robert C. *Whatever Possessed the President? Academic Experts and Presidential Policy, 1960–1988* .... David M. Welborn 751
Baumgartner, Frank R., and Bryan D. Jones. *Agendas and Instability in American Politics* .... Christopher J. Bosso 752
Bledsoe, Timothy. *Careers in City Politics: The Case for Urban Democracy*
Williams, Shirley, and Edward L. Lascher, eds. *Ambition and Beyond: Career Paths of American Politicians* .... Michael B. Berkman 753
Brace, Paul, and Barbara Hinckley. *Follow the Leader: Opinion Polls and the Modern Presidents*
Mann, Thomas E., and Gary R. Orren, eds. *Media Polls in American Politics*... Beth Reingold 754
Brecher, Charles, and Raymond D. Horton. *Power Failure: New York City Politics and Policy Since 1960*
Mollenkopf, John Hull. *A Phoenix in the Ashes: The Rise and Fall of the Koch Coalition in New York City Politics*............. Terry Nichols Clark 756
Brehm, John. *The Phantom Respondents: Opinion Surveys and Political Representation*.. Judith M. Tanur 758
Ellis, Richard J. *American Political Cultures* .... Michael Brint 759
Epstein, Richard A. *Bargaining with the State* .... Marc Eisner 760
Fowler, Linda L. *Candidates, Congress, and the American Democracy* .... John R. Hibbing 761
Gillman, Howard. *The Constitution Besieged: The Rise and Demise of Lochner Era Police Powers Jurisprudence*
Richards, David A. J. *Conscience and the Constitution: History, Theory, and Law of the Reconstruction Amendments*....... David M. O'Brien 762
Hattam, Victoria C. *Labor Visions and State Power: The Origins of Business Unionism in the United States*......................... Michael Goldfield 764
Hero, Rodney E. *Latinos and the U.S. Political System: Two-tiered Pluralism* .... James A. Regalado 765
John, DeWitt. *Civic Environmentalism: Alternatives to Regulation in States and Communities*
Ringquist, Evan J. *Environmental Protection at the State Level: Politics and Progress in Controlling Pollution*....................... Norman J. Vig 766
King, Richard H. *Civil Rights and the Idea of Freedom* .... Richard M. Valelly 768
Palazzolo, Daniel J. *The Speaker and the Budget: Leadership in the Post-Reform House of Representatives*........................... John J. Coleman 769
Ripley, Randall B., and James M. Lindsay, eds. *Congress Resurgent: Foreign and Defense Policy on Capitol Hill* .... Cecil V. Crabb, Jr. 770
Rosenstone, Steven J., and John Mark Hansen. *Mobilization, Participation, and Democracy in America* .... Kristi Andersen 771
Rush, Mark E. *Does Redistricting Make a Difference? Partisan Representation and Electoral Behavior*............................. Gary L. Crawley 771
Sniderman, Paul M., et al., eds. *Prejudice, Politics, and the American Dilemma*... Howard Schuman 773
Thomas, Sue. *How Women Legislate* .... Diane D. Blair 774

COMPARATIVE POLITICS

Bustamante, Jorge A., et al., eds. *U.S.-Mexico Relations: Labor Market Interdependence*.................................... Philip L. Martin 776
Diamond, Larry, ed. *Political Culture and Democracy in Developing Countries*... Peter McDonough 777
Hadenius, Axel. *Democracy and Development* .... R. Kent Weaver 778
Holmes, Leslie. *The End of Communist Power* .... John P. Willerton 779
Jackman, Robert W. *Power Without Force: The Political Capacity of Nation-States* .... Crawford Young 780
King, Ronald F. *Money, Time, and Politics: Investment Tax Subsidies and American Democracy*
Steinmo, Sven. *Taxation and Democracy: Swedish, British, and American Approaches to Financing the Modern State*......... Michael Wallerstein 781
Koven, Seth, and Sonya Michel, eds. *Mothers of a New World: Maternalist Politics and the Origins of Welfare States*.................. Gretchen Ritter 782
Manion, Melanie. *Retirement of Revolutionaries in China: Public Policies, Social Norms, Private Interests* .... Hong Yung Lee 783

Martin, Curtis H., and Bruce Stronach. *Politics East and West: A Comparison of Japanese and British Political Culture* .... Suzanne B. Ryan 784
Marx, Anthony W. *Lessons of Struggle: South African Internal Opposition, 1960–1990* .... Robert Fatton, Jr. 785
Merkl, Peter H., and Leonard Weinberg, eds. *Encounters with the Contemporary Radical Right* .... Michael Minkenberg 787
Miller, Arthur H., William M. Reisinger, and Vicki L. Hesli. *Public Opinion and Regime Change: The New Politics of Post-Soviet Societies* .... Ellen Mickiewicz 788
Ross, Marc Howard. *The Culture of Conflict: Interpretations and Interests in Comparative Perspective* .... Richard Ned Lebow 789
Shirk, Susan L. *The Political Logic of Economic Reform in China* .... Avery Goldstein 790
Silberman, Bernard S. *Cages of Reason: The Rise of the Rational State in France, Japan, the United States, and Great Britain* ...... David H. Rosenbloom 791

INTERNATIONAL RELATIONS

Armstrong, David. *Revolution and World Order: The Revolutionary State in International Society* .... Beth A. Simmons 793
Armstrong, Tony. *Breaking the Ice: Rapprochement between East and West Germany, the United States and China, and Israel and Egypt* .... Stephen R. Rock 794
Campbell, David, and Michael Dillon. *The Political Subject of Violence* .... J. Ann Tickner 795
Cooper, Andrew F., et al. *Relocating Middle Powers: Australia and Canada in a Changing World Order* .... David P. Rapkin 796
Crawford, Beverly. *Economic Vulnerability in International Relations: East-West Trade, Investment, and Finance* .... William J. Long 797
Dittmer, Lowell. *Sino-Soviet Normalization and Its International Implications, 1945–1990* .... Ilpyong J. Kim 798
Forbes, Ian, and Mark Hoffman, eds. *Political Theory, International Relations, and the Ethics of Intervention* .... David A. Welch 799
Forsythe, David P. *Human Rights and Peace: International and National Dimensions* .... Nils Petter Gleditsch 800
Kurzer, Paulette. *Business and Banking: Political Change and Economic Integration in Western Europe* .... John Zysman 801
Mazlish, Bruce, and Ralph Buultjens, eds. *Conceptualizing Global History*
  Woo-Cummings, Meredith, and Michael Loriaux, eds. *Past as Prelude: History in the Making of a New World Order* .... Ali A. Mazrui 802
Midlarsky, Manus I., ed. *The Internationalization of Communal Strife* .... Herbert K. Tillema 804
Onuma, Yasuaki, ed. *A Normative Approach to War: Peace, War, and Justice in Hugo Grotius* .... Nicholas Onuf 805
Renshon, Stanley A., ed. *The Political Psychology of the Gulf War: Leaders, Publics, and the Process of Conflict* .... Louis Kriesberg 806
Rosecrance, Richard, and Arthur A. Stein, eds. *The Domestic Bases of Grand Strategy* .... Richard K. Betts 807
Ross, Robert S., ed. *China, the United States, and the Soviet Union: Tripolarity and Policymaking in the Cold War* .... June Teufel Dreyer 808
Smock, David R., ed. *Making War and Waging Peace: Foreign Intervention in Africa* .... Roy Licklider 809

POLITICAL THEORY

Postmodernism and Democratic Theory. By Aryeh Botwinick. Philadelphia: Temple University Press, 1993. 288p. $39.95 cloth.

Postmoderns are commonly criticized for what Jürgen Habermas has called a "performative contradiction": in order to attack modern reason they must also employ it. As Botwinick showed in his earlier *Skepticism and Political Participation* (Temple, 1990) tensions between the implicit presuppositions and the explicit formulations of philosophical arguments—or problems of self-referentiality—are hardly unique to postmodernism. However, postmodernism is his focus here. The central question of *Postmodernism and Democratic Theory* can be formulated as follows: How might a self-reflective postmodern read postmodernism today? Botwinick presents his response in two parts. The first addresses epistemological-metaphysical issues and the second turns to their political-economic analogues. Throughout Botwinick stresses relationships between theory and practice, especially the historical convergence of postmodern philosophies and liberal-democratic traditions.

In Part One, Botwinick reformulates several criticisms of skepticism to suggest that thinking always begins "in the middle" and continually "unmasks itself" (p. 6). Perhaps most striking is his challenge to Habermas's distinction between tacit knowledge and communicative rationality, both of which are lifeworld phenomena. Botwinick argues that speech-act theory, including Habermas's, is best construed as a consistent pragmatism: sentences work simply because they work. Habermas, he claims, recognizes the cultural origins and limitations of his supposedly "universal pragmatics." He acknowledges them indirectly through his fascination with Benjamin's concept of "Jezt-Zeit" or "now-time." "Jezt-Zeit" refers to the convergence of past and future in the present moment. According to Botwinick, it is an "ultimacy which refuses to take its own ultimacy for granted," which accepts our presence in the world as a "protracted stationary moment (pp. 88–89)." So understood, Habermas's theory of communicative rationality is considerably closer to the ethos of postmodernism than his criticisms suggest.

Botwinick calls the epistemology which corresponds to our existential condition a "generalized agnosticism." It presupposes that "we are creatures living in time," whose insights, however profound, are ephemeral, and who simultaneously create and destroy meaning(s). This epistemological stance allows Botwinick to employ Levenson's distinction between tradition, as "an unselfconscious acceptance of a way of life," and traditionalism, as the "self-conscious affirmation of tradition in the face of competing alternative" (p. 2). Traditionalism, he argues, is quite compatible with postmodern critiques of modernity: "Under a generalized-agnostic dispensation we notice (or confer) value without ever leaving the plan of a continually expanding present" (p. 10).

In Part Two, Botwinick presents the political analogue to this epistemology. A "generalized-agnosticism" is enacted and institutionalized through the always unfinished character of democratic participation. Botwinick argues that what he names a "postmodernist liberal communitarianism" corresponds to the existential condition and the economic circumstances of humanity today. Regarding the former, a dual emphasis on participatory and procedural aspects of democratic traditions allows the expression of deeply rooted identities within a framework of individual rights. With respect to the latter, Botwinick suggests that through democratic conversation, i.e., by "wagering" ourselves in community with others, modern individuals can learn to accept conditions of scarcity and to affirm existence "divorced from concerted exploitative interaction with the world" (p. 229).

The above overview only scratches the surface of a complicated and sophisticated argument. Botwinick supports his position with interpretations of numerous philosophers, e.g., Plato, Hobbes, Rousseau, Freud, Wittgenstein, Lyotard, Strauss, etc., besides those mentioned above. At times, his many shifts between authors and texts give the book a disjointed quality. I found Botwinick most effective when he analyzed metaphors to illustrate how philosophers can reveal our present limitations, e.g., his discussion of Hobbes's *Leviathan* to show how spatial constructions of representation create conflict under conditions of scarcity. With such analyses, Botwinick performs the intellectual's vocation which he, following Edward Shils, defines as the "sacralization of existence" through the "attaining of the greatest possible degree of coherence about oneself and the world" (p. 175). Still, I wish Botwinick had given less weight to theoretical arguments. In the introduction, he says, "History in the 1990s seems to be keeping pace with and embodying the latest insights of theory, so that what might appear as abstract philosophical argument in the pages that follow has its 'payoffs' in the current shape of political reality" (p. 2). Like Botwinick elsewhere, I doubt our ideas have such power to shape the events with which they are always already intertwined. For that reason, I hope he will develop empirical applications of his argument (perhaps along the lines of David Kolb's *Postmodern Sophistications: Philosophy, Architecture and Tradition*, [1990]), to tell us even more about our "continually expanding present." Where is the participatory democratic alternative to the "political spectacle" of representative government which Murray Edelman has so powerfully described (1989)? According to Botwinick, it is there we should seek our present salvation.

Pennsylvania State University NANCY S. LOVE

Democratic Community: NOMOS XXXV. Edited by John W. Chapman and Ian Shapiro. New York: New York University Press, 1993. 400p. $42.50.

This volume marks the end of John Chapman's distinguished editorial participation in the NOMOS series. It carries on the characteristic virtues of its predecessors: first-rate contributors representing a wide range of views, engaged debate, painstaking documentation of sources, and careful editing. The editors are to be congratulated, as well, for a detailed index and a brisk but serviceable introduction. I missed only a brief consolidated bibliography, which would have facilitated classroom use.

Taken as a whole, the collection represents a state-of-the-art meditation on relations, theoretical and practical, among a familiar triad of themes: communitarianism, liberalism, and democracy.

We are offered what is in part a continuation, in part the termination, of the liberal–communitarian debate. On the termination front, Alan Ryan argues (conclusively, I believe) that the debate between "atomistic" and "socially constituted" conceptions of the self does not map neatly onto the quarrel between liberal and illiberal conceptions of social relations. One can, without contradiction, combine a holistic view of the individual and a rights-based or liberty-based view of politics. (This point has been conceded by Charles Taylor.) Not surprisingly, Ryan goes on to sketch a "communitarian liberalism" in which our complex social interdependence is deployed on behalf of an individuality that goes beyond the fulfillment of social roles.

I am struck by how few of the symposiasts even tried to define *community*. Yet this is not wholly without reason: by 1955, a rural sociologist had already catalogued 94 different senses of the term and proposed a minimum condition (common locality), only to have his suggestion rejected by nearly all later scholars. Robert Post cites Michael Sandel: a community is a form of social organization that provides its members not just relationships they *choose* but attachments they *discover*, "not merely an attribute but a constituent of identity" (p. 165). Berry argues that this constitutive interpretation of communal identity poses risks to individual liberty. Jane Mansbridge defines *community* as a group "in which the individual members can trust other members more than they can trust strangers not to 'free ride' or 'defect' in social dilemmas, not to exploit the members of the group in other ways, and, on occasion, to further the perceived needs of the group rather than their own needs" (p. 340). Carol Gould suggests a "minimal sense" of community as agents who have a "common interest in shared ends, in pursuit of which their cooperation is voluntary and not merely constrained by law or habit, or effected by coercion," reserving judgment about the centrality of the more affective and self-sacrificing bonds Mansbridge stresses (pp. 398–99).

Underlying and animating this volume is worry about the authoritarian risks of communal life. Undemocratic communities typically succumb to these tendencies. But does a democratic organization of political life by itself suffice to protect individual freedom? (Carmen Sirianni poses just this question in his revealing account of the vicissitudes of participatory governance in the feminist movement.)

According to one line of thought, this question is poorly posed: the only freedom that counts is the freedom to participate fully and equally in the making of the laws under which all must live. The contributors to this volume vary in their stance toward this Rousseauian claim, but none seems entirely comfortable with it. The reason is familiar but not trivial. At least two other kinds of liberties seem to have independent moral weight: (1) equal protection in the ways laws are applied and enforced, through police powers, courts, and juries; (2) privacy in the broad sense—the right to be left alone, the enjoyment of a zone insulated against public (and social) interference.

Amy Gutmann has long argued that properly understand as a core commitment to collective deliberation and individual autonomy, democracy entails the protection of many classic liberal freedoms. She continues that line of argument in her contribution to this volume. She insists that the greatest freedom is the freedom to "deliberate and decide political issues consistent with the like freedom of every other adult member of one's society"; but she acknowledges that the tension between liberalism and democracy does not entirely disappear: democractic deliberation may result in the subordination of a range of personal freedoms to other social goods (p. 152). She is not unduly troubled by this, because she regards the subordination of these personal freedoms as legitimate when produced by, or in the service of, public deliberation (p. 154).

Many staunch liberals will, of course, continue to take a different view. Gutmann's move is, in effect, challenged by contributors such Christopher Berry and Kenneth Minogue, who see a much more troubled relationship between the demands of community and the requirements of individual liberty as they understand it.

Bruce Rutherford's essay asks whether undemocratic groups can contribute to the process of democratization. He poses this question concerning Muslim fundamentalism in Egypt. But it raises a broader issue, which Rutherford traces back to Tocqueville: What stance should democratic nations adopt toward subcommunities whose internal norms and governance structures do not conform to democratic equality as currently understood? A newly dominant view in the United States is that these groups should be tightly regulated and compelled to democratize through the coercive power of the federal government. Rutherford tacitly gives us reasons to reconsider this policy.

University of Maryland WILLIAM A. GALSTON

Burdens of Proof in Modern Discourse. By Richard H. Gaskins. New Haven: Yale University Press, 1993. 362p. $35.00.

With the contemporary erosion of faith in scientific, religious, and political authority, our attention turns naturally to the conditions of public discourse. Are the forums within which putative experts seek the redemption of their claims (whether to truth or to moral rightness) sufficiently unconstrained? Do institutional judgments ultimately rest upon the pragmatic force of the best argument? Richard H. Gaskins argues that they do not but suggests that they could.

In presenting an argument-from-ignorance, a speaker contends that the inability of the opponent to refute the thesis constitutes sufficient grounds for accepting it. Rather than developing a positive case for the thesis, the speaker presumes its truth and shifts the burden of proof onto the opponent. Logic textbooks rightly treat the argument-from-ignorance as fallacious, but Gaskins contends that it nevertheless pervades modern discourse.

Gaskins specifically treats dialectical discourse, defined as argumentation oriented toward decision. He locates dialectic midway between logic and rhetoric.

Like rhetoric, dialectic does not possess the apodeictic certainty of logic. Nevertheless, like logic but unlike rhetoric, dialectic seeks both finality and legitimacy in its judgments. In order to secure finality and legitimacy, dialectical tribunals develop doctrines that distribute to one side the presumption (e.g., of truth); to the other side they allocate the burden of proof. The side with which the presumption rests enters the argument greatly advantaged, for it can rely on the argument-from-ignorance. Their opponents, on the other hand, bear the burden of proof and so must present a positive argument.

In the American legal system, the burden of proof rests invariably with prosecutions in criminal cases, while defendants enjoy the presumption of innocence. Civil courts distribute the burden of proof to plaintiffs and the presumption of nonliability to defendants. But in constitutional law, Gaskins argues, the Supreme Court exercises considerable latitude in distributing presumptions and burdens. Reviewing primarily cases involving due process and affirmative action, Gaskins identifies such assignments as the principal instrument with which recent Supreme Courts condition the outcomes of subsequent case law and thereby effectively establish public policy.

Burden-shifting strategies appear likewise in academic and other professional discourses. Gaskins shows burden shifting to be the primary discursive strategy of the Critical Legal Studies movement in particular and among deconstructionists more generally. The movement's representatives offer few reasoned justification for their heterodox views. Indeed, most of them deeply suspect that claims to reasoned justification are rhetorical camouflage for ideological preferences. In exposing these preferences, deconstructive critics shift burdens of proof onto prevailing orthodoxies that would otherwise claim presumptive truth for their positions. Whether critical legal scholars acquired this technique from their law school mentors (as Gaskins asserts) or the practice reflects a rhetorical tradition more deeply embedded in Western scholarship (as he argues later), Gaskins rightly locates the argument-from-ignorance at the heart of the deconstructive enterprise.

Examining the relationship between public regulatory policy and science, Gaskins questions whether the latter requires quasi-judicial tribunals to adjudicate scientific issues that impinge on public policy decisions. In the postpositivist period, of course, science will not issue final judgments. At least, scientific judgments cloaked in the claim to finality would, for most scientists, lack legitimacy. In the post-Popperian era, moreover, science now finds itself incapable even of pointing to a body of thus far irrefuted hypotheses. Where scientists divide on scientific issues with policy relevance (e.g., regarding ecological matters), advocates on either side of a policy debate can muster scientific support in order to defer to their opponents the burden of proof. Gaskins observes that by failing to institutionalize scientific tribunals, advocates of judicialized science lost an opportunity to impose finality upon factual disputes involving scientific questions. Yet he does not consider the obvious dangers thereby avoided.

Scientific authority might be salvaged (for scientists, if not for policymakers) by relocating scientific truth to a postulated transcendental plane beyond the reach of practical human activity. Ethical authority, after all, has risen by descending from just such a plane. But the liberal theorists whom Gaskins reviews postulate their own, idiosyncratic transcendental tribunal for ethical judgment. On the earthly plane of practical judgment (as in the decisions of bioethics panels), this diversity creates immense scope for strategically allocating presumptions and burdens.

Gaskins finds in Kant's *Critique of Pure Reason* the foundational source for burden shifting. In his famous antinomies, Kant deploys parallel arguments-from-ignorance to establish the strict separation of scientific and ethical discourse—of facts and values. Only now, two centuries later, have we begun to breach this attractive, yet fundamentally mistaken, conceptual divide. Hegel first recognized the error in his later work, and Gaskins devotes his final chapter to an exigesis of the framework for recovering reason embedded in Hegel's *Science of Logic*—a framework, Gaskins reveals, that guides his exposition from the beginning. While Gaskins's account of Hegel's dialectical logic is difficult, most readers will find it far more accessible than the original.

In the end, Gaskins' readers will want more. They will wish, for instance, that he had returned, armed with his dialectical method, to the practical controversies discussed earlier in the book. How, specifically, would the dialectical approach resolve or otherwise affect the controversies that rage currently in the law schools? How would it affect constitutional interpretations or ethical judgments regarding medical practices? Some readers will also wish Gaskins had considered a wider range of controversies. How would a dialectical understanding of scientific controversies affect the ad hoc tribunals that review manuscripts for publication, particularly in the theoretically pluralistic social sciences? Moreover, some might wish Gaskins had attended to argumentative discourses—like negotiations—that occur in the absence of arbitrating tribunals. Do presumptions and burdens shift with greater rapidity where no institutional mechanisms exist for enforcing them? Would a dialectical understanding of rational controversy among the parties help to produce outcomes that each would find just?

This wish list reflects more the provocative nature of Gaskins's contribution than any real deficiency. The exposition is elegantly crafted and difficult to put down. Its concerns address many of the specialities of our discipline. Students of constitutional law will find illuminating Gaskins's accounts of the judicial discourse surrounding affirmative action and due process. Political theorists will find utility in his comparisons of contemporary liberal theories. Students of bioethics and of public policy, particularly environmental policy, will find helpful his accounts of discursive strategies in their fields. Finally, the work will also prove useful to those political methodologists who recognize their responsibility to attend to the discursive conditions under which truth claims are argued and warranted.

Syracuse University GAVAN DUFFY

Making Men Moral: Civil Liberties and Public Morality. By Robert P. George. New York: Oxford University Press, 1993. 241p. $42.00.

Making Men Moral is a worthy contribution to perhaps the most constructive debate taking place in political theory today. George asks whether the liberal state can respect individual freedoms *and* act legitimately to preserve a moral climate in which citizens live, even if the latter requires legislation that criminalizes certain "victimless" actions. More broadly, George suggests that liberal freedoms themselves only make sense against the background of a theory of the human good and thus offers a perfectionist account of civil liberties.

George's moral concerns are shared by communitarian critics of neo-Kantian liberalism and, increasingly, by liberals, as well. The vision of the neutral state presented by John Rawls, Ronald Dworkin, Bruce Ackerman, and others has begun to strike more and more readers as neither a good empirical description of what the liberal state does nor a good ideal of what it should do. The state is not (nor should it be) indifferent to the ethical question that guided roughly two thousand years of preliberal thought: What is the good life for humankind, and how is it best promoted?

George seems to be moderately liberal in his political inclinations—a fellow traveler, at least, though he does not fit his own rather odd definition of the term (p. 167). Indeed, he bends over backward to assure his audience (whom he clearly presumes to be more liberal than he) that his theories do not lead to draconian restrictions of civil liberties that some may fear. Yet like Joseph Raz, William Galston, and others, he critiques major neo-Kantian liberals (including Rawls and Dworkin) for failing to offer an explicit account of the good but then slipping a suspect individualist notion of it into their theories of justice (p. 133). He also allows for a degree of moral paternalism on the part of the state that neo-Kantians would reject. It must be said, however, that George is too reluctant to point to anything beyond the most obvious examples of the types of moral laws that may be justified. Indeed, of his staple examples (prostitution, drug abuse, and pornography) two out of three are illegal in most jurisdictions, and the third is not afforded full free-speech protections even by those who would not accept his perfectionist premises in theory or practice. One suspects that there is more at stake in his arguments than he is willing to let on in this work.

Be that as it may, George's arguments are consistently intelligent and challenging. In his view, what ultimately grounds civil liberties to speech, press, privacy, worship, and so on is that they advance intrinsic human goods. Each of these liberties is an instrumental good, so that, for example, speech "that fails to advance any human good is valueless, for the value of speech is instrumental, not intrinsic" (p. 195). Such a view opens the door to morals legislation on the grounds that speech, association, and so on that are used for vicious, rather than virtuous, purposes are entitled to no legal protection. Such legislation, the author contends, cannot make men moral, because morality must come from within; but it can dissuade people from pursuing—maybe even considering—"the grosser forms of vice," which is a legitimate function of the state (p. 72).

George takes the side of Lord Devlin in his classic debate with H. L. A. Hart in arguing that there is no sphere of privacy necessarily protected from state power—or, as the author puts it, there is no intrinsic right to do wrong. However, George dissents from Devlin's view that public morality (and thus permissible morals legislation) is essentially a matter of public opinion. Rather, the rationally ascertainable truth of the proposition that a given practice is immoral is essential to the determination that the practice should indeed be outlawed.

How successful is George's perfectionist liberalism? I must register some doubts. George goes to great lengths to reassure liberals that though his political theory draws upon what he calls the central tradition of Western political thought from Aristotle through Aquinas, it escapes the potential threats to liberty found in these teachings. It does so largely be recognizing the great diversity of basic goods (and thus of morally worthy practices), which, in turn, assures that not much can be banned by the state after all. The great failure of the central tradition, George argues, was its blindness to this moral pluralism and thus its coming to decidedly antiliberal conclusions reflected both in theories of liberty and in social structure.

Yet George's "correction" of the central tradition's defects relies on diversity to do too much theoretical work. It is a misreading to suggest that Aristotle lacked "an appreciation of the diversity of human goods" and thus of good way of life open to people (p. 39). No reader of the *Nicomachean Ethics* could take this claim seriously. Aristotle believed, however, that such goods are internally related (so that the virtues are a package) and that these goods can be ranked. For example, one cannot simply say that friendship is among the basic human goods, so that one may choose the life of fealty to friends as one form of the best life. Friendship not informed, for instance, by a sense of justice can become quite vicious indeed. The problem here is that George cannot simply stop short a perfectionist rendering of the human good to better fit it to liberal sensibilities. A real coherence to Aristotle and Aquinas's thought leads to antiliberal conclusions that an admirer like George should not so lightly dismiss.

There are other ways in which George seems to compromise his argument to make it more palatable to liberal critics. He consistently relies on prudence to expand the scope of liberties, forgetting that it is in the nature of prudence to cut in more than one direction. In one revealing passage, he suggests that theory aside, it might be unwise to relinquish too much morals enforcement power to the modern legislator, given "what it takes to achieve public office" today and the flawed people these conditions place in power (p. 40). For a heartier perfectionist, such a denunciation might lead to a critique of our way of choosing leaders, perhaps of democracy itself. For George, it is a fact to which his theory should be accommodated.

George's most fundamental (and most heartfelt) reasons of principle push him to restrict civil liberties to advance the good, while prudence (and one weak argument on diversity) pulls him back. This is not enough for many liberals to jump on his bandwagon. Yet this closely reasoned book is more than enough to lead them to think about why they value liberty as much as they do, and this is all to the good.

University of California, Davis RICHARD C. SINOPOLI

The Descent of Political Theory: The Genealogy of an American Vocation. By John G. Gunnell. Chicago: University of Chicago Press, 1993. 330p. $47.00 cloth, $17.95 paper.

Gunnell tells a story, beginning in the mid-nineteenth century, of the emergence of political theory as an academic field of study. The account he offers is an "internal" one, focusing on the writings of notable political scientists and theorists, examining how they characterized their enterprise, criticized each other, and appropriated the work of their predecessors and contemporaries. According to Gunnell, the central preoccupation of political science has always been its relationship to political practice. Political scientists have consistently sought to provide the kind of knowledge necessary to sustain, or perfect, democracy, though their understandings of both democracy and the knowledge it requires have been contested.

Until the 1940s, there was no clear definition of political theory as distinct from political science. At various times, it even disappeared as a classification of fields in the publications and programs of the American Political Science Association, and there was little consensus about its content, methods, or role in the discipline. There were, of course, debates in political science that foreshadowed the "behavioral reformation" (as Gunnell calls it) of the 1950s, most notably Merriam's call for a more "scientific" study of political life, one that would generate reliable knowledge that would give political science the necessary authority to address policymakers. But Merriam himself was trained as a theorist and continued to teach the history of political ideas throughout his career. His vision did not require the repudiation of political theory nor even a clear understanding of what it involved.

The critical event, in Gunnell's view, was the arrival of German scholars during the 1930s. Gunnell's discussion of this period—the range of intellectual positions they held, how their concerns articulated with those of American political science, and the reception of their ideas—is among the most original and interesting parts of this work. The German émigrés, notably Strauss and Voeglin, did not share the commitment of American political science to liberal democracy. Indeed, they viewed liberalism (particularly in the pragmatist version that was popular at the time) as part of the problem and launched a powerful attack on the whole idea of political science as it had historically been understood. Their attack was joined by many American theorists, such as John Hallowell, who had also studied in Europe. This attack, Gunnell argues, precipitated the so-called behavioral revolution. But this was not so much a revolution as a reaffirmation of the historical commitment to the role of political science as providing the knowledge necessary to the realization of democracy. Far from a radical break with traditional political science, the behavioral movement thus represented a conservative response to the European challenge, albeit one that one-sidely emphasized the "scientific" understanding of the enterprise.

Although Gunnell's story is fascinating and insightful, it relies too heavily on the role of Easton's work in launching the behavioral movement and on the conflict at Chicago between Strauss and his more scientifically inclined colleagues. Considering the virtual irrelevance (not to say unintelligibility) of the Straussian critique to most political scientists outside of the field of political theory, it is hard to believe that Easton's appeal could be explained in terms of the challenge of the German émigrés and their American followers.

The challenge of Strauss also figures prominently in the final descent of political theory, its marginalization within political science, and its de facto transformation into an interdisciplinary field including practitioners from various academic disciplines. Noting that the principal response within political science to Strauss came from Wolin and his colleagues at Berkeley, Gunnell argues that the real significance of the émigré challenge was to initiate a contest for the soul of political theory. In the end, the Straussians lost, but political theory emerged alienated both "from politics and from itself," becoming a purely academic discipline, without any connection to political practice (p. 274). Political theory lost even its distinct role of providing a "reflective and critical voice" for the discipline of political science and had become hostage to the discourses of philosophy or the history of ideas (p. 269).

Gunnell had advanced this characterization of political theory in a series of publications over the past decade, but it is not made more plausible in this work than it was before. It is certainly true that political theory has become much more autonomous and more closely linked to other fields, but that development characterizes many fields within the humanities and the human sciences. There is both more specialization and more boundary crossing of conventional department lines; and both developments are, at least arguably, desirable.

One of the main weaknesses of the overall argument of the book is that the concept of the relationship between theory and practice—between the study of politics or political theory and political life—is not analyzed very closely. Gunnell offers one model of that relationship in the person of Francis Lieber, who associated with the "public figures of his time" (p. 25), but it is not clear that he did so to much effect. In any event, universities have become an important source of personnel for political office, particularly in the executive branch of the national government.

A second model is that political science might be an important source of ideas for public policy. There is an obvious tension between democracy and expertise; but if we do not suppose that the academy is to speak with a single voice, imposing its authoritative knowledge on the policy, the university can contribute to democratic life by generating ideas and subjecting them to critical scrutiny. Gunnell complains that political theory fails to speak to public issues and that its language is too esoteric to be accessible to a broader public. No doubt, that is true of much academic political theory, but his discussion (confined largely to the story of the "failed journal," *democracy*) is too limited to support that claim.

These weaknesses result, in part, from the fact that Gunnell's history is an "internal" one, examining the conversations of political scientists and theorists among themselves. This is a valuable approach, and makes an important contribution to our understanding of the development of the discipline. But it is not designed to capture either the impact of external events on the discipline or how the work of its practitioners may have been appropriated by others, including citizens and policymakers.

Wesleyan University J. DONALD MOON

A Theory of Liberty: The Constitution and Minorities. By H. N. Hirsch. New York: Routledge, 1992. 276p. $49.95 cloth, $15.95 paper.

H. N. Hirsch has written an engaging book presenting a sustained argument for the *liberty theorem,* which holds that the Court should strike down laws that unduly restrict liberty even if the passage of those laws has met the requirements of procedural due process. With lively writing, gripping examples, discussion of cases not often discussed in general treatments of the Supreme Court (most notably civil commitment cases), and a concluding chapter that attempts to relate constitutional law to the contemporary liberal–communitarian debate in political theory, Hirsch provides a welcome addition to liberal jurisprudential thought. Hirsch does not shy away from taking a stand. He is mostly supportive of Justice Brennan's decisions and bitingly critical of Justice Rehnquist, who at one point he accuses of applying an "extraordinarily tortured logic" that results in an opinion filled with "nonsense" (pp. 175, 177). Hirsch does not rely on such rhetoric, however; he offers reasoned arguments that are often quite convincing.

That said, I want to point to some difficulties I find with Hirsch's liberty theorem. The liberty theorem holds that "liberty can be restricted only for agreed, limited purposes" (pp. 40–41, cf. 131). Where a law unduly restricts liberty, the Court may strike it down. One of the textual grounds for this argument is the Due Process Clause of the Fourteenth Amendment: "Nor shall any State deprive any person of life, liberty, or property, without due process of law." Most laws deprive us of liberty in some way: the phrase "without due process of law" is therefore a crucial qualifier. Hirsch's liberty theorem demands substantive due process: according to it, the mere passage through fair democratic procedures of a liberty-restricting law that is neither vague nor unfairly applied is not sufficient to satisfy the demands of the due process clause.

What, then, are the substantive requirements for a liberty-restricting law if it is to be constitutional on Hirsch's view? What does due process require? The text of the Constitution is unclear, and we lack "clear 'proof' of specific framers' intent" (at least of the due process clause), so Hirsch turns to "the general ideological background of the constitutional period" and of the Republican party in the decades leading up to the passage of the Fourteenth amendment (pp. 31, 55, 60, 66). Hirsch shows that Montesquieu, Locke, and Blackstone all emphasize the importance of freedom from "arbitrary government action" (chap. 2). Blackstone, for example, opposes restrictions on liberty that are "wanton and causeless" (p. 40). The liberty theorem, then, requires that a law abridging liberty not be arbitrary, or "wanton and causeless." Regulating behavior with such a law denies due process of law no matter how scrupulously fair were the procedures in passing and enforcing the law.

Blackstone's criterion that laws not be wanton is easily met by most laws, as is another of his criteria, that laws restricting liberty must not be "without any good end in view" (p. 40). The laws curtailing liberty at issue in *Lochner*, *Roe*, and virtually all of the other cases that Hirsch discusses were not pointless: legislators had (nonarbitrary) reasons for passing these laws. We need some further criteria for deciding what counts as an "arbitrary" law. Locke and Montesquieu do not help us much with this question. Hirsch's point in turning to them as sources of authority in constitutional adjudication is unclear to me, especially since Hirsch ends up turning (in effect, though not explicitly) to John Stuart Mill instead. Hirsch's project can be described as in part the constitutionalization of Mill's harm principle (which holds that my liberty may be legitimately restricted by the state only when necessary to prevent me from harming others).

As the argument proceeds, the requirement that a liberty-curtailing law not be arbitrary becomes more stringent: to satisfy the (substantive) demands of due process, such a law, it turns out, needs "extraordinary justification" that "can only come in the form of (a) reliable empirical evidence of concrete harm, or (b) a unanimous or nearly unanimous moral sentiment" (p. 100). The freedom to engage in homosexual sodomy or to enjoy sexually explicit books and films should, on Hirsch's account, be constitutionally protected against restrictive laws because empirical evidence shows that the expression of these freedoms does not cause harm, and there is no nearly unanimous moral consensus for such laws (see pp. 99–100, 103 [n. 88], 146).

It is hard to see why originalists or strict textualists should be persuaded by this position. We might expect Dworkin, who is neither, to argue that if we think these principles of due process derive from the best political philosophy, we should use them to interpret an unclear constitutional text. But Hirsch, in chapter 1, criticizes Dworkin, in part because he finds Dworkin so ahistorical. Yet it is hard to see how Hirsch's constitutionalization of the harm principle is itself historically grounded.

Even as a moral argument, the liberty theorem is troubling. Liberties that do not cause harm to others are not obviously all equal: some seem more basic than others, and their constraint seems to demand more justification. The liberty of speech and the right to vote seem more worthy of respect than the liberty to use narcotics; and some forms of the liberty of sexual expression (homosexual sodomy) may be worthy of respect than others (viewing hard-core pornographic films). Such distinctions are controversial but should at least be considered in a legal theory about liberty. Even if we accept as worthy of constitutional protection *all* nonharm-producing liberties, we still need to decide what counts as harming others. There is a growing body of thought on this problem (most notably the work of Joel Feinberg) that Hirsch does not address. Hirsch's other criterion for upholding a liberty-restricting law is that the law reflect a nearly unanimous moral judgment. Hirsch implies that the Court should uphold a law like Georgia's that prohibits homosexual sodomy if "the sentiment against homosexuality were as strong as the sentiment against murder," which, he says, is not the case (p. 146). But if the due process clause or other liberty-protecting clauses provide substantive protection of liberties or rights against majoritarian impulses, why—morally or constitutionally—should the level of consensus on restrictive laws determine the level of protection these liberties or rights receive?

Hirsch's book is to be commended first as an engaging introduction to important constitutional issues and second, for providing a sustained argument that liberty is the guiding principle of the Constitution. Hirsch has laid the groundwork for further consideration of whether Mill's harm principle should be given constitutional effect.

Stanford University MARK TUNICK

Postmodern Contentions: Epochs, Politics, Space. Edited by John Paul Jones III, Wolfgang Natter, and Theodore R. Schatzki, New York: Guilford, 1993. 210p. $18.95 paper.

Politics, Theory, and Contemporary Culture. Edited by Mark Poster. New York: Columbia University Press, 1993. 300p. $55.00 cloth, $17.50 paper.

Perhaps it is an exaggeration to say that the postmodern epoch came into its own when the Berlin Wall was torn down. But certainly, the event symbolized an emerging consciousness of a future opened to new possibilities but also a future that, perhaps necessarily, cannot be mapped, least of all by a triumphant liberal–democratic ideology that has come to mean everything to everyone. We are caught in a moment in which ideas—no longer frozen by the Cold War—have a window of opportunity. And yet events are overtaking ideas at a pace that threatens to leave us only with retrospective reflections.

The essays collected by Mark Poster reflect this sensibility. They are "postmodern" in their view that received metanarratives (e.g., liberal, Marxist, neoconservative), no longer tell us what to prefer and how to choose. These essays are less concerned to show that the various disciplines that provide initial focus (literary criticism, political theory, feminism, urban studies, and psychology) are "political" than with rethinking what "the political" has become. The best essays do precisely this. In perhaps the most original and theoretically ambitious essay of the volume, Ernesto Laclau examines whether the "exhaustion of the great narratives of modernity" underwrites a retreat from politics by robbing politics of its meaning. This view, he notes, assumes that the meaning of political activity reduces to its representation of a grand narrative. In fact, there is retreat not from politics but (more specifically) from revolutionary designs unwritten by grand modernist narratives. Politics is, as it were, relocating itself within the "ontology of the social"; that is, politics is increasingly local, contingent, and part of the fabric of society. It is often radical but no longer revolutionary. So we need to reconceive politics generically, as "the ensemble of the decisions taken in an undecidable terrain" within social relations generally, which Laclau conceives as sedimented power relations that have blurred their own contingency. In such a context, Enlightenment ideals do not lose their meaning but, rather, become "pragmatic social constructions" that reorient those social relationships that have become "political," that is, visibly held together only by power (p. 295). The relationship between politics and meaning can emerge locally and contingently, just insofar as political settlements allow "positive communitarian" goods to develop within a terrain that had been undecidable (p. 296).

In much the same vein, Jean-Francois Lyotard, reflecting on the Gulf War, suggests that struggles for emancipation must now take on the character of immanent critique, rather than wholesale opposition. What we ought to favor is the "openness (or looseness)" of systems, which will tend to survive those that claim a "closed" spiritual center such as Iraq. An open system "no longer needs to be legitimated on a metaphysical basis. Rather, it only needs the free space" within which critique and argument can thrive (p. 270). One wonders whether Lyotard (had this essay been written after Desert Shield gave way to Desert Storm) might have seen residues of metaphysics in "the system's" sometimes genocidal conduct.

Other essays are less synoptic. An intriguing essay by Samuel Weber finds a postmodern ethos in Lacan's "Ethics of Psychoanalysis." Whereas traditional ethics seeks external containments of desire, Lacan finds an ethics in desire itself. Roughly, only limits to desire can constitute an other that can be desired. But "desire for the other" involves breaching these limits, meaning that desire is inherently tragic in its mode of satisfaction. There is an ethos inherent in this dilemma, namely a recognition of the other as irreducibly different, something that cannot be subsumed. In contrast, traditional ethics have a "constitutive incapacity to respect limitation and, in particular, *self-limitation*" (p. 140). Such ethics justify an excess of external power against desire that defeats the more subtle if tragic dialectic of desire and, with it, a self-limiting subject who is open to difference. Two other competent if more limited essays probe psychological issues: Grabriele Schwab criticizes Fredric Jameson's *Political Unconscious* for reducing the sexual elements of desire to economic desires, thus missing a terrain that is central to feminism. And Teresa de Lauretis reads Freud's *Three Essays on the Theory of Sexuality* to show that even in Freud's terms, heterosexuality is but one possible organization of the drives. "Normal" sexuality is normative and not natural (p. 126).

In a subtle and attentive essay, Catharine Stimson asks about the political implications of feminism's increasing reflexivity and maturity, noting that in shifting away from grand narratives—a shift she does not regret—feminism has yet to find a substitute means of galvanizing a feminist movement. An innovative if not entirely successful essay by John Carlos Rowe reinterprets Marx's concept of class by focusing on the increasing importance of symbolic values in an era of information intensity; high technology; paper debt, credit, and wealth; and symbolically produced and manipulated needs. Class, Rowe argues, is now less determined by industrial location than by location within symbolic structures. Political agency now requires new intellectual skills of symbolic manipulation. In an essay entitled "Community after Devastation," David Carroll asks whether community is possible after the Holocaust, in which the rhetoric of community covered for massive atrocities. In surveying several attempts to rethink community as a space of public criticism, Carroll rejects the language-based approaches of Habermas and Arendt (arguing, incorrectly, that these approaches are based on a metaphysics of consensus) in favor of Jean-Luc Nancy's view that literary criticism provides the kind of reflexive horizon that works against closure. This essay reflects a failure—not uncommon among those whose academic home is literary criticism—to distinguish questions about self-making and consciousness raising from recommendations for political institutions, notwithstanding obvious interrelations. It is possible to institutionalize conversations if conditions favor talking over fighting. But what can it mean to institutionalize politically the consciousness of a work of art? The medium is inappropriate. Finally, in a typically masterful performance, Jacques Derrida reflects on a series of travel narratives on Moscow (his own included) in order to show how they operate as projections of utopian hopes and fears.

The essays collected by Jones, Natter, and Schatzki are good (if not pathbreaking) essays. They are careful, disciplinary reflections. They amount to taking stock—surveying the existing terrain of academic discourse in political theory, philosophy, literary studies, geography, and anthropology. If they add up to a single point (one intended by the editors), it is that we should resist the polarities of the modern/postmodern divide, just because the discourses are so interwoven that they do not fall out along these lines without leaving a remainder. In their introduction, the editors trace themes that are key to postmodern styles (the crisis of the subject and representation, the need for irony, the overfullness of life) as far back as Schlegel and Novalis, while pointing to the political events today that have revived such reflexive critique of Enlightenment. Although an interesting survey, the editors lapse into the intellectual historian's tracing of themes, a method that always turns up antecedents and is thus incapable of identifying originality. An essay by Fred Dallmayr is more precise: he views postmodernism as an internal critique of modernity, which results in "a radical relativism in which no part can claim absolute primacy or supremacy" and which for this reason coheres with a radically democratic ethos (p. 34). Dallmayr rejects Lyotard's definition of postmodernism as "the rejection of metaphysics," since reversals too easily repeat metaphysical formulas. Likewise, in an interview with the editors, Richard Bernstein argues that the modern, postmodern polarity obscures more than it reveals: Habermas, for example, is a thoroughgoing critic of the Enlightenment, while Derrida has certainly never argued against "reason." Quoting Albrecht Wellmer, Bernstein suggests that "postmodernism at its best might be seen as a self-critical—a skeptical, ironic, but nevertheless unrelenting—form of modernism" (p. 103). In a rich and suggestive essay, Edward Soja notes likewise that postmodernism is a living dynamic that can be identified only as reactions to successive modernist hegemonies, as far back as 1789. This does not detract from the uniqueness of contemporary postmodernism, occasioned by the emergence of a symbolic hyperreality. As a geographer, Soja is especially concerned to develop a critical theory of space, a project he locates by noting that human existence is defined simultaneously within spatial, historical, and social dimensions. With the important exceptions of Foucault and Henri Lefebvre, Soja suggests that the spatial dimension has been neglected within critical theory and postmodern writings. In an essay comparing postmodern writings in literary criticism and geography, Wolfgang Natter and John Paul Jones III argue that weaknesses of postmodern styles tend to mirror the disciplines: literary criticism has neglected space, while geography has neglected representation. In a disciplinary survey of anthropology, Catherine Lutz notes that feminist anthropologists (who have been developing postmodern approaches since the early 1970s) were marginalized and unacknowledged as postmodern styles were initiated into the anthropology establishment in the early 1980s. In a clever set of comparisons, Lutz indicates the extent to which "establishment" postmodernism feeds off of simple reversals of male/female stereotypes (p. 156).

Three essays address the politics of postmodernism directly. Soja draws useful distinctions among a postmodernism of the Right that feeds off of symbolic manipulation, a "fence-sitting" postmodernism that glorifies the "play of signifiers" without developing any particular commitments, and a postmodernism of the Left that aligns itself with radical democracy. A lucid and sensible essay by Theodore Schatzki compares Foucault and Lyotard, suggesting that Foucalt's radically liberal commitments complement Lyotard's emphasis upon systematic oppression. An essay by Ernest Yanarella provides a careful comparison of Derridan and Gramscian politics, while recommending the kind of synthesis developed by Ernesto Laclau and Chantal Mouffe in *Hegemony and Socialist Strategy*.

Taken as a whole, these collections provide articulate thoughts on emerging political terrains, as well as useful guides to emerging academic reflections.

Georgetown University MARK E. WARREN

The Morality of Pluralism. By John Kekes. Princeton: Princeton University Press. 1993. 227p. $29.95.

This book is about not the moral significance and consequences of political, social, or cultural differentiation but, rather, a more abstract issue internal to much contemporary moral and political philosophy: How do values stand in relation to one another, and how are conflicts among them to be resolved?

Value pluralism, according to Kekes, has several components—indeed, six theses: pluralists (1) "deny that there is an authoritative system of values and, consequently, that there is any value that is always overriding" (p. 19); (2) take value conflicts to be unavoidable, because values can be both incompatible and incommensurable (though incompatible values need not be incommensurable and vice versa); (3) believe in "reasonable conflict-resolution," holding that conflicts can, for the most part, be rendered tractable through "balancing" against the background of an accepted tradition; (4) take values to be "possibilities of life" that we may "reasonably" value, albeit many of them are incommensurable and incompatible; (5) recognize the "need for limits," defending some values against others on "context-independent grounds" (esp., "deep conventions" that "protect the minimum requirements of all good lives," p. 34); and (6) entertain the prospect of "moral progress" in relation both to individuals and to traditions. For individuals, "the ideal is to construct for ourselves a reasonable hierarchy of the possibilities we value" (p. 36). Moral progress occurs in a tradition that is "as receptive to a plurality of conceptions of a good life as is consistent with the limits needed to maintain that tradition" (p. 35).

Kekes's value pluralism is a descendant of the idea first launched by Sir Isaiah Berlin in his various writings, and then taken up by a variety of philosophers. But unlike Berlin, Kekes seeks to underplay the tragic dimension of pluralism and to deny the claim that there is any affinity (or even compatibility) between pluralism and liberalism. On the other hand, like Berlin, he tries to distinguish pluralism from both monism and relativism.

Monism he takes to be the claim that there is either a summum bonum (as Plato and Aquinas thought) or a medium in whose terms values can be ranked (as utilitarians and subjectivist theorists of values as preferences maintain) or a canonical principle for ranking values (which can take a stronger or weaker form, the latter coming close to pluralism). The key contention of pluralists against monists, to which Kekes accords overriding importance, is that no value or set of values can be *overriding*.

The other flank of Kekes's attack is relativism. What distinguishes relativists from pluralists, he says, is that "relativists conclude that the plurality of values excludes the possibility of objective justification and criticism of values, while pluralists believe otherwise" (p. 46). Relativism can, of course, be more or less *radical,* depending on whether they hold all values to be context-dependent or only some. Kekes's general way with relativists is to insist on "context-independent grounds" for what he calls "primary values" (benefits that are universally human and part of all conceptions of a good life) and "deep conventions."

Kekes's argument is a tightly woven fabric, though it is not highly colored by examples, either of the thickly described anthropological or historical sort or the thinly described philosophical sort. It also has many loose ends. There is no extended discussion of key notions essential to his argument, such as "reasonableness"; the distinction between primary and secondary values is insufficiently and even inconsistently specified (sometimes the latter are said to be interpretations of the former); and the idea of "balancing" is never properly explained (an odd omission, since it incorporates the metaphor of "scales," yet the values to be weighed are said to be incommensurable). Nor is the very notion of incommensurability adequately treated, though, in this respect, Kekes is at one with almost all contemporary writers who have appealed to this notoriously slippery notion.

Least satisfactory of all is the concluding discussion of the relations between pluralism and liberalism. Kekes argues that there are good reasons for holding that these are incompatible. (By liberalism he means the contemporary political–philosophical versions.) His central reason is that liberals are committed to "overriding values," whereas pluralists are not. But overridingness is another notion he both fails to explicate and uses inconsistently. Sometimes he seems to mean the according of priority to a value, irrespective of social context. Yet Rawls, for example, is very clear that his principles of justice only come into play beyond a certain level of social and economic development. Moreover, it is clear, at various points in his argument, that Kekes systematically gives priority to the value of autonomy (e.g., p. 215). Nor is it evident what the "politics of pluralism," as Kekes sketchily describes it, would actually look like. Nor, finally, is it clear how the state could "take an active role in advocating very many substantive values" (p. 215) if these are incompatible and incommensurable.

European University Institute STEVEN LUKES

Reconsidering the Democratic Public. Edited by George E. Marcus and Russell L. Hanson. University Park: Pennsylvania State University Press, 1993. 478p. $55.00 cloth, $13.95 paper.

The chronic and usually debilitating tensions that exist in political science between "theorists" and "empiricists" have been fruitfully utilized in this book to produce a series of interesting and imaginative essays on problems of democracy. The book is an outgrowth of an extended discussion among the scholars held at the Democratic Theory Symposium at Williams College during the summer of 1989. To enhance reader interest and understanding, each leading essay is followed by an extended critical analysis.

In the lead chapter, Benjamin I. Page and Robert Y. Shapiro agree with the commonplace view that the opinions of ordinary people on important issues are ill informed, irrational, and unstable. But they argue that collective public opinion (as distinguished from individual opinion) is real, rational, and stable. This is so because collective public opinion comprises the statistical aggregation of individual opinions. More important, the authors emphasize that the collective public opinion is influenced and shaped by intercommunication among governmental, social, and media elites and among elites and the public. The collective public opinion is declared rational and stable because it responds in a reasonable way to changing realities and ideas and this public opinion tends to form coherent patterns that are attached to American values.

Benjamin Barber is less than satisfied with the Page and Shapiro study because, among several reasons, they fail to investigate the key issue whether public opinion is an outgrowth of citizen discourse, judgment, and deliberation or (as the determinists would have it) shaped by external forces. Unwittingly, Barber argues, the authors tipped the scales in favor of determinism by using a language of opinion, private interest, elites, and individualism, rather than a democratic vocabulary that underscores the importance of citizen participation.

The cleavage between a normative and an empirical orientation to politics was more successfully breached in the Conover, Leonard, and Searing essay. To contribute to a clearer understanding of what constitutes a democratic conception of citizenship, the authors conducted an empirical study to determine whether liberal and/or communitarian concepts of rights and duties are influential in the way citizens think about their rights and responsibilities of citizenship. It is no surprise that most of the people tested had a liberal understanding of rights, an understanding that underscores the importance of civil rights, in contrast to social rights. Similarly, the common understanding of responsibilities of citizens is basically liberal in character; that is, citizen responsibilities are primarily limited to those necessary to preserve negative rights. Within this context, it is not difficult to understand why Americans tend to be indifferent, if not hostile, to the plight of the homeless. The authors point out that irrespective of whether people embrace liberal or communitarian attitudes, when it comes to "particular situations," they find themselves in agreement on what constitutes appropriate action. Because of the readiness of citizens to reconcile differences when it is deemed expedient to do so, the authors suspect that liberal–communitarian debate is not fruitful toward reaching an adequate conception of citizenship. In a sharp analysis of sexual violence and liberalism, Virginia Sapiro and Mary Lyndon Shankley call for a restructuring of both the theory and the practice of democracy as an essential means to achieve equality of citizenship between women and men.

It is encouraging that as many as four of the essayists were able to draw "cautiously optimistic" conclusions that segments of the American political system that they studied were in good health. Donald Kinder and Don Herzog bolstered and enriched, Page and Shapiro's claim that owing to the "miracle of aggregation," collective public opinion is indeed "extremely sensible [and] reasonable" (p. 371). In back-to-back essays John Kingdon and Jane Mansbridge reaffirmed that ideas are not necessarily tied to self-interest and that they can be significant forces in shaping political outcomes. And Jennifer Hochschild reenforces the judgment of "cautious optimism" about the system by showing that disjunction and ambivalence, for a multiple of reasons, act as a democratic catalyst to the deliberation process.

Judged individually, the quality of the essays in this book range from good to outstanding. However, it is deficient in the one fundamental respect that it is incremental in its vision: it focuses exclusively on various parts of the system, not on the whole. Thus, ironically, it is unable to address (much less answer) the most crucial question, namely, How healthy is American democracy?

Temple University PETER BACHRACH

Beyond Relativism: Science and Human Values. By Roger D. Masters. Hanover: University Press of New England, 1993. 248p. $24.95 cloth.

Twelve years ago, Thomas C. Wiegele published a book on an emerging revolution in social sciences. He argued that new findings in biological sciences made such a revolution inevitable. Masters's new book demonstrates that this revolution is really taking place. He challenges some basic premises of modern science, including social sciences, and provides ideas and material for a new paradigm. His targets include, among others, Locke's tabula rasa model of the brain, the long held ideal of value-free science, scientific value relativism, cultural determinism in social sciences, the nature-versus-nurture dichotomy, and the so-called naturalistic fallacy. His major thesis seems to be that the gulf between *is* and *ought* is not unbridgeable and that as ancients taught, knowledge of nature is at the foundation of human values and goals.

The book is divided into three parts. In the first part, Masters describes the nature of science and makes comparisons between modern science and ancient science. One very interesting insight is the assumption that the friendship between Leonardo da Vinci and Niccolò Machiavelli was a central event in the emergence of modernity and of modern scientific method. In the second part, he discusses the nature of facts and values and argues against relativism. He differentiates three models of knowledge—intuition, hypothesis testing (or empirical verification), and pattern matching—and rejects the relativistic or nihilistic assertion of postmodernism, according to which "humans are alone in an alien universe with no inherent principles of value or meaning" (p. 8). Masters thinks that it is possible to discover objective knowledge by either verification or pattern matching and that knowledge provides a factual basis informing our understanding of what is good for humans "according to nature." He connects the three models of knowledge to distinct processes in the brain: the limbic system, the left hemisphere, and the right hemisphere. In the third part, he tries to build a bridge from facts to values—from the naturalistic fallacy to naturalism—and to integrate nature and nurture into a new paradigm in the social sciences.

Masters's essay is based on very extensive interdisciplinary material and evidence. Notes covering 80 pages in the end of the book provide an eloquent demonstration of his enormous expertise, which ranges from ancient philosophers to modern science (esp. political science, political theory, biopolitics, evolutionary biology, behavioral ecology, and neuroscience). Bibliographical notes provide rich information of relevant literature.

The significance of this book is in its attempt to prove that some major premises of social sciences are obsolete and that we need a new paradigm. Let us look at one point in which the contemporary environmental paradigm of social sciences is clearly obsolete. His argument that Locke's tabula rasa model of the brain is wrong is based on convincing evidence. He claims that "the behaviorist model of the brain has been demolished by recent research" (p. 74). There are innate ideas in the brain. Phenotypic behavior is always a result of the interaction between genetic and environmental factors. Social sciences, however, have limited their attention to environmental factors.

One could say that Masters succeeds better in the demolition of the obsolete environmental paradigm of social sciences than in the formulation of a new paradigm based on the principles of naturalism, but he makes it clear that the new paradigm should reflect the integration of nature and nurture and the possibility to derive values from facts. On the basis of this book, it is not yet easy to see how values can be derived from knowledge of nature and human nature, although he gives some examples. He says, for example, that because of the natural inevitability of differences in perception and judgment, we should expect and respect individual differences (p. 123). Consequently, tolerance of others is a moral virtue. His conclusions on the moral virtues of abortion and infanticide are more provocative. He argues that because Darwinian principles lead to variation in the extent to which mammals care for their young, we cannot establish any absolute moral values concerning abortion and infanticide. They are adaptive strategies that vary depending on the environment. Therefore, we can only say that "abstinence is preferable to contraception, contraception is preferable to abortion, abortion is preferable to infanticide, infanticide is preferable to murder, and murder is preferable to war and plague" (p. 128).

Many readers will probably disagree with Masters in some major or minor points. Let us take some examples. He refers to a contradiction between competition and cooperation. However, one could argue that there is no contradiction between them. Cooperation is just another evolved mechanism of competition. Usually people cooperate to further their selfish interests. He does not pay much attention to assumed evolved predisposition to reciprocity. However, it is quite possible that many values are based on expectation of reciprocity. He discusses the genetic basis of individual variability in behavior convincingly, whereas he pays less attention to the existence of numerous behavioral universals and their relevance from the perspective of human values. Donald E. Brown's *Human Universals* (1991) might have been useful.

This is a very important theoretical book. It is true that Masters does not formulate a complete new paradigm, but he introduces a research project intended to produce a new paradigm for social sciences. We need theories and hypotheses that combine evolutionary and environmental factors. The integration of the theories and findings of new biological research into a new paradigm of social sciences is an enormous task. The process of change has been slow for the reason that most social scientists are still ignorant of the relevant research outside their own branch. Masters's book helps to correct this lack of information. It is worthwhile reading for political scientists interested in the basic premises of our branch of science.

University of Helsinki TATU VANHANEN

Profits, Priests, and Princes: Adam Smith's Emancipation of Economics from Politics and Religion. By Peter Minowitz. Stanford: Stanford University Press, 1993, 345p. $45.00.

Any account of this book has to ignore the alliterative but largely decorative title and concentrate on the subtitle. Even so, the reader will find this overdramatic, because what Peter Minowitz actually wishes to maintain is that Adam Smith was engaged in a process of "deflecting," rather than emancipating economics from politics. What Smith was doing to religion is rather more complex: "disguised subversion" seems the best description. Nor is it always entirely clear whether or not Minowitz wishes to maintain that deflection and subversion were self-conscious aims on Smith's part. More often, it seems as though he wishes to claim that Smith began a process that was "consummated" in the work of his more positivistically minded successors as economists and social scientists, with Marx being given a final chapter as the consummate consummater.

Although this study of Smith begins and ends by situating itself against the background of the demise of communism, Marxism, and the "end of history" (with many other excursions into current affairs introduced along the way, especially those associated with contemporary environmental dilemmas), it is as a contribution to Smith scholarship that the work will mainly be judged here. Many of the book's theses could be sustained without reference to Smith, but a particular reading of Smith is the chosen vehicle on this occasion. In this choice (if not in how he conducts his detailed argument), Minowitz is working with the grain of an established stereotype. Economists may famously disagree about most things, but they are usually united on the paternity of their discipline; and it is not difficult to obtain their agreement on the related proposition that the act of emancipating economics from politics (let alone from religion) ought to be celebrated, rather than regretted. Minowitz does not collude with the one-eyed habit that sustains such professional hubris by confining himself to the *Wealth of Nations*. Rather, he deals with the problem of reconciling this work with the *Theory of Moral Sentiments* and comes close to reviving an influential account of the relationship advanced by Jacob Viner in 1926 and withdrawn later; namely, that the providentialism of the moral philosophy is at odds with the Godless realism of the political economy. Minowitz's version of the thesis that there truly is an "Adam Smith Problem" is more complex and pays more attention to religion. Thus, both works are held to contain neglected contradictions within and between themselves.

In other respects, however, Minowitz departs from the usual perspective adopted by historians of economics as a positive science. He believes that Smith "facilitated the *continued* process of narrowing of political economy" (p. 3, my emphasis)—an observation that could just as well be replaced by the statement that in repossessing political economy, Smith broadened it by placing it within the context of a secularized version of natural jurisprudence and moral philosophy that emancipated it from the restrictive mechanical and political arithmetic cages within which it had previously been confined. Smith's enterprise, taken as a whole, was as generously proportioned as his physiocratic contemporaries and far more so than any of those predecessors he castigated as the "pretended doctors" of the mercantile system. It also entailed a break with "narrow" (Hobbesian or Mandevillian) conceptions of economic man. What has usually preoccupied historians of economics has been how Smith's *successors* (Jean-Baptiste Say, J. R. McCulloch, and Nassau Senior) formally announced the divorce of political economy from politics, criticizing the master for overinclusiveness. For Minowitz's metahistorical purposes, such details can presumably be overlooked.

Announcements that Smith was responsible (usually single-handedly) for deflecting politics in an economistic direction are more common in histories of political philosophy written during the second half of this century. The works of Joseph Cropsey (*Polity and Economy*, 1957) and Sheldon Wolin (*Politics and Vision*, 1960) provide distinguished earlier examples; but a longer list of younger followers could be compiled, some (but by no means all) of whom appear in Minowitz' bibliography. Though critical of Cropsey's insights on matters of detail, Minowitz seems closest to him in deploying Straussian criteria for deciding in what respects Smith is responsible for replacing or undermining concepts that are essential to any truly political standpoint. There may also be a Straussian warrant for the underlying historiography of the work, judging what Smith was doing in terms of what he licensed later authors to do and even in employing a version of Strauss's "esoteric" approach to texts: on religious matters especially, Minowitz maintains that there is a subterranean agenda beneath Smith's writings that he may have been following covertly, even unconsciously.

Since Smith is being judged throughout more by what he was *not* trying to do (or was neglecting to do), it is essential to have some clear criteria by which this record of failure or avoidance can be judged. In this respect, Minowitz did specify the standards by which Smith can be deemed unpolitical or apolitical, almost a positivist *avant la lettre*. The notorious difficulty of establishing negative propositions (the infinite number of nonconclusive possibilities) is turned into an advantage. Here are some of the political and religious things that Minowitz finds absent by the chosen criteria. Smith "says nothing about promoting the peaks of courage and intelligence," nor does he "endorse a ruling class or extol a particular set of political institutions"; he "never presents a typology of regimes, much less an evaluation of the different forms of government" (pp. 19–21). Normative conceptions of justice and equality are absent. Biblical injunctions, the after-life, and Jesus Christ are missing, and morality is deprived of sanctity. Whereas Rousseau is "brimming with political philosophy," Smith's cup is almost empty.

When Plato, Aristotle, Socrates, Cicero and others are brought into the picture, the *nowheres* and *nevers* fly thicker and faster. Even the "joy of sex" is deprecated by Smith's "nonerotic prose" (with Nietzsche, rather than

Alex Comfort, providing the relevant standard here). Smith says "nothing to encourage war" and little to foster "national pride." Even Smith's placement of commas and use of footnotes is invested with negative significance. When "complexity" is recognized, it is said to approach "unintelligibility" (p. 79). Only the "bourgeois virtues" of economic man are left, with the result that Smith becomes Machiavelli's economic alter ego and Marx's flawed predecessor.

Alternatively, Smith is presented as an atheistic proto-Darwinian, concerned only with opulent survival and species propagation, the author of a vision of order without design that leaves only "paradoxical vestiges of traditional perspectives" (p. 133). What seems at stake here is no less than "mankind's hubristic drive for cosmic sovereignty" (p. 239). Smith is judged not to be politically correct on the environment. Priests, but not political economists, are prevented from exploiting workers, leading to the sweeping conclusion that "the study of Smith's texts is unlikely to vanquish the disorienting and demoralizing effects of modern capitalism" (p. 254).

With so much of Smith left on the floor after this compulsive chipping-away process, the result was perhaps predictable. Less heroically normative and more catholic conceptions of political speculation and debate would have yielded very different results. Serious consideration of Smith's *Lectures on Jurisprudence* might also have led to a different destination. Smith is too cool a customer for those who yearn for life in warmer climes. Yet if, as Minowitz is forced to conclude, we have no alternative but "to muddle through," Smith's understanding of what it is like to prescribe for a world that is always second best may be a valuable starting point. I have made little direct reference to all the modern problems laid at Smith's door, but one final reflection is provoked by Minowitz' belief that technology and the rampant tendencies of capitalism continue to lie at the heart of world disorder. Perhaps the skeptical Smith, together with his atheistic friend David Hume, understood something about the political dangers of religious factionalism and "enthusiasm" that Northern Ireland and Bosnia (and other recent East European events) have brought to our attention once more.

University of Sussex DONALD WINCH

Not by Reason Alone: Religion, History, and Identity in Early Modern Political Thought. By Joshua Mitchell. Chicago: University of Chicago Press, 1993. 252p. $32.00 cloth.

Joshua Mitchell argues that the major early modern political theorists began from an interpretation of history rooted in biblical interpretation. He focuses his attention on four thinkers: Luther, Hobbes, Locke, and Rousseau. He thinks that virtually no contemporary scholar realizes that early modern political theory requires an appreciation for the interconnections between politics, history, and religion. In particular, contemporary readings of these thinkers distort their views by evaluating them as either liberal or antiliberal, a distinction that would not have been meaningful to them. The proper question, according to Mitchell is, "What kind of individual or community does the thinker develop from his theory of history?" The key notion in this investigation is that of "politically authoritative history," which Mitchell explains as "that history invoked, the implications of which authorize or legitimate a particular political order, extant or envisioned, which simultaneously *situates* psyche or the soul in a narrative it *knows* to be true yet might wish to evade" (p. 2).

In addition to an introduction and conclusion, there are four chapters, each devoted to one of his four protagonists. The chapter subtitles identify Luther's dialectic as one of "Supersession and the Politics of Righteousness," Hobbes's as one of "Renewal and the Politics of Pride," and Locke's as one of "Clarification and the Politics of Reason." All three dialectics allude to the meaning of Christianity in relation to the pre-Christian. Rousseau, not being Christian, has no dialectic: "The History of Diremption and the Politics of Errancy."

According to Mitchell, the structure of Luther's authoritative history consists of three parts: the carnal world of the Old Testament, the spiritual world of the New, and the revelation that will occur at the end of the world. This view seems to me to be the generic Christian view. The carnal world is superseded by the spiritual, and the revelation at the end of the world will supersede the spiritual in the Hegelian sense that what supersedes something incorporates and transforms it into something better. In the current state of the world, secular authority is necessary because the carnal world has not been obliterated. Mitchell constructs convenient dichotomies to characterize the carnal and spiritual worlds. The carnal world is active, the spiritual passive; the carnal exterior, the spiritual interior; the carnal relies on good works, the spiritual on faith. These dichotomies strike me as ripe for deconstruction, and Mitchell has an inkling that they are oversimplifications.

Mitchell sees Hobbes as a Christian thinker, whose central worry is how to eliminate the snares of pride. Obedience is the answer. Mitchell calls this a Reformation theme. Again, this element seems to me to be a part of generic Christianity. The moral of the story of the first sin is that all of the miseries of humanity came into the world because Adam and Eve disobeyed God.

Most scholars underemphasize the religious dimension of Hobbes's thought. Mitchell overemphasizes it. He treats the political thought of Hobbes with hardly a mention of the state of nature and no discussion of the laws of nature or the logic of the social contract. Mitchell is correct to see Leviathan as having quasi-divine characteristics but wrong to claim that Leviathan is "the shadowy, mysterious sovereign who never shows his human face" (p. 59). The illustrated title page of *Leviathan* is worth a thousand words of refutation. Mitchell claims that Christ is a central political figure for Hobbes. I think that Hobbes's insistence that Christ came to give counsel, not commands, and that he did not establish a kingdom show that Hobbes depoliticized Christ.

Locke differs from Luther and Hobbes in that for him, there is no tension or conflict between religion and government. Rationality is the principle of politics, and reason dictates that religion and government be separate. This is the basis for Locke's stance on toleration. The dialectical element in Locke's thought is supposed to be the idea that the Old Testament clarifies the New. This continuity between the two does not seem to me to merit the title "dialectical."

Mitchell points out that the reason that Filmer and not Hobbes is Locke's main target in the *Two Treatises* is that for Filmer, Adam is an individual human being, who received political authority from God, while for Locke, Adam represents all humanity. On Filmer's view, subjects should be treated as children—hence, his patriarchalism. In contrast, on Locke's view, subjects are adults, competent to make their own decisions. Adam's role is "clarified" by Christ, not superseded. He has, then, no special political role to play.

Rousseau's thought is wholly secular in content even though it retains the form of biblical history. It lacks Christ; and that makes all the difference in the world, according to Mitchell. There can be no dialectic between the promise and fulfillment of redemption or the presence and absence of Christ. For Rousseau, there are two states, which Mitchell takes to be historical categories: the state of nature and the civil state. The state of nature is Edenic but lost. The civil state is deficient and cannot be stable for Rousseau, because he cannot appeal to the salvation brought by Christ. Rousseau invents the general will as the savior of people. Mitchell thinks that Rousseau's problem is the same as the one posed by Christian thinkers and that Rousseau cannot solve it because he does not have the resources of Christianity. Assuming that Rousseau's problem is structurally similar to the Christian problem, I do not see why each cannot be seen as instances of a general problem for humans, which is not peculiarly Christian. Mitchell does not consider that possibility.

The text is roughly 150 pages long; the notes occupy about 85 pages. They often seem only tangentially relevant to the text. They could have been pruned by at least half.

This book may appeal to those who talk in terms of trace, diremption, privileging, errancy, and unconcealment. Others will probably find the reading slow and the argumentation lacking or dubious.

University of Texas, Austin A. P. MARTINICH

The Moral Economy of Labor: Aristotelian Themes in Economic Theory. By James Bernard Murphy. New Haven: Yale University Press, 1993. 244p. $25.00.

This is a conceptually powerful book that deserves a wide audience. Despite its title (a better one might have been simply *The Moral Foundations of Work* or *Work, Human Nature, and Morality*), the book is not just for those interested in labor theory or Aristotle. Nonetheless, it fits with a reviving inquiry into Aristotle's economic views—for example, William James Booth's (1993) *Households: On the Moral Architecture of the Economy* (with which it also shares common territory in Marx) and articles by Abram N. Shulsky, Darrell Dobbs, and Scott Meikle.

Besides its lucidity and erudition, what makes Murphy's book compelling reading for all kinds of political theorists (if not all political scientists) is its two forceful and broad conceptual clarifications: of the relation between moral and technical reason and of the relations among nature, custom, and law or deliberately stipulated norms. Abstracted from Murphy's main theme—the character of productive labor—these fundamental philosophical distinctions are in themselves illuminating and will illuminate other problems in political theory and other fields. By clarifying the complexity of the overlapping relations between "the natural order of physical, chemical, and biological processes, . . . the customary order of habitual social practices, . . . and the stipulated order of deliberate design," for example, Murphy exposes the oversimplicity of the nature/convention dichotomy and the sophistic nature/nurture or nature/culture debate it has generated, providing a significant intellectual service that should influence research in many fields, from anthropology to sociology (p. 46).

The book's central concern is no less an original and profoundly insightful contribution to existing scholarship. Following Aristotle, Murphy argues that the exercise of our faculties is fundamental to human flourishing. Reconstructing Aristotle, he argues that even productive labor or economic work should therefore aim to unify the tasks of conception and execution. But it is neither Murphy's call to humanize labor in the workplace (found in his prologue) nor his practical proposals for how to do so (found in his epilogue) that are new or the focus. Similar and related calls and proposals have recently been put forth in edited collections like Nussbaum and Sen's *Quality of Life* (1993) and Ellen Frankel Paul and her colleagues *Economic Rights* (1992). By contrast, Murphy's project—an important contribution to the history of economic theory and must reading for anyone whose work invokes Marx or Smith—is to account for *why* dehumanizing divisions of labor are the norm in both capitalist and communist countries. We must diagnose the disease, which lies in economic theories put forth by thinkers on both the Right and the Left, before we can see the cure.

According to Murphy, degrading divisions of labor exist because they have been theoretically justified in the name of efficiency by classical political economy and classical political thought. Political and economic thinkers from Plato to Taylor failed to conceive a division-of-labor theory that promoted the self-development of each worker because they were handicapped by two erroneous assumptions. First, they assumed that efficiency requires not simply the division of tasks (the technical division of labor) but also the assignment of those tasks to individual persons (the social division of labor). Not grasping the two senses of the division of labor, they assumed that the efficiency criteria were the same for both, blinding them to the insight that productivity is in part a function of worker morale. Second, they thwarted the discovery of a humane division of labor because they assumed that efficiency requires dividing labor according to natural differences, such as age, sex, race, and ability. They failed to see that although efficient divisions of labor must make use of natural powers, custom and stipulation often determine the natural differences on which such divisions of labor are based. Even where custom and stipulation select natural aptitude as the basis for job distribution, matches between jobs and persons are imperfect because our aptitudes are shaped by custom and stipulation.

According to Murphy, then, nature (and thus human nature) is plastic: "All human activities are subject to natural causal laws, but to a considerable extent, custom and stipulation select which causal laws govern a particular activity" (p. 62). As in Stephen Salkever's important *Finding the Mean: Theory and Practice in Aristotelian Political Philosophy* (1990), Murphy indicates that natural and social science can share a fundamental unity of method that is both explanatory and evaluative. Accordingly, Murphy's analysis avoids the democratic or populist mistake of reducing our resistance to nature to mere choice or will or of implying that morality is a matter simply of choice making: "Nature creates potentialities that are actualized by custom but also imposes restrictions on the emergent customs; customs open possibilities for deliberate stipulations but also limit those possibilities" (p. 61).

Murphy's thinking thus again harmonizes with Aristotle's. Although, Murphy observes, Aristotle did not posit a divine personality as the source of natural law, he transformed the traditional concepts of universal unwritten divine law and of the particular unwritten law of equity "into a doctrine of natural law," making a "signal contribution to legal theory" that anticipated Stoic and Thomistic natural law doctrine (p. 141). Murphy thus importantly narrows, without closing, the gap that Leo Strauss sees between Aristotle and Aquinas. What Murphy and adherents of Strauss's interpretation do not consider, however, is that paradoxically, Strauss's own interpretive method (i.e., resolving a philosopher's conflicting statements by giving more weight to less frequently than to the more abundantly made statement) highlights Aristotle's explicit reference to natural law in the *Rhetoric* against his discussion of natural right in the *Nicomachean Ethics*.

An overall analysis of Murphy's book reveals, however, a problematic tendency: although he expertly presents the intricate reciprocities between nature, custom, and rational stipulation, he tends (or appears ultimately) to accord rational stipulation the upper hand over nature and its pull through custom. Even if the skeptical reader sets aside doubts that most productive labor can be redesigned to include a stimulating conceptual dimension and that such redesigned work can motivate the unmotivated, one wonders if promoting self-development through challenging work might not—by cultivating pride, competitiveness, or ambition in workers—threaten, rather than achieve, the resulting "solidarity" in the workplace that Murphy anticipates. He criticizes Marx for predicating social solidarity on a homogenized species beingness, instead of on differentiation; but at least Marx understood that we cannot perfect both individuation and social solidarity.

In the epilogue (the only disappointing part of the book) the paradox of Murphy's larger argument crystallizes. By sketching tired and unproven communitarian proposals for promoting better jobs—unions, workplace democracy, and education of workers in engineering (to gain mastery over conception)—Murphy inadvertently compels the reader to recall his earlier devastating critique of the pseudoscience of society management called "political economy": since that science presupposes "a collective subject that acts to maximize social welfare" (the "communist fiction" that society is organized into a single household or firm), it "points inexorably to socialism," "in short, is possible only in the absence of politics" (p. 145). Surely, individuation increases politics (as Athenian democracy proved), which in turn threatens the communist fiction on which rest the "strong and progressive" unions that Murphy says are "a minimal prerequisite for self-realization at work" (p. 230).

The main problem, then, with Murphy's self-realization-through-work argument is that (ordinary) work is done *at* work, with multiple others. To achieve the level of social harmony we need to get work done at the workplace, we need to compromise and to some degree suppress the very drives (which the Greeks called *eros* and *thumos*) that fuel self-realization or excellence. If work has such a political dimension that impedes the pursuit of excellence or development of our true selves, then the logical alternative is to develop ourselves through nonpolitical activities. Addressing in his prologue the possibility that leisure might become the primary sphere for the realization of human potential, Murphy points out that there is a fruitful dynamic between work and leisure: "Challenging work leads to challenging leisure" (p. 4). While the habits and skills developed at work may enrich leisure, might not leisure (as Plato and Aristotle argue) develop different and higher skills that most ordinary work cannot? And might not the attitudes and opinions assimilated at work—especially at deliberately democratized work—discourage self-realization after work? Indeed, Nietzsche may be right that stipulated or mandatory communality cultivates a herd mentality that discourages the pursuit of differentiating activities. Thus, although the call to make work more developing of the self is well intentioned, perhaps the democratic tendencies or demands of work frustrate, as much as they foster, the realization of our highest individual potentialities.

Boston University JUDITH A. SWANSON

The Nature of Rationality. By Robert Nozick. Princeton: Princeton University Press, 1993. 226p. $19.95.

The Concept of Political Judgment. By Peter J. Steinberger. Chicago: University of Chicago Press, 1993. 300p. $53.00 cloth, $17.95 paper.

On the face of things, these books form a good pair to review together; in fact, they make a very odd couple: readers who find the style of argument of both books to their taste are likely to be rare. They reflect the odd way that the label "political theory" is used in political science—as (1) a commentary on, and continuation by close reading of, texts of the long tradition of philosophical reflection on politics and (2) the often mathematical analysis of politics as the aggregate effects of the narrowly rational choice of individuals responding to incentives, modeled after, and descending from, economic theory.

Nozick is a philosopher best known for a work about politics (*Anarchy, State, and Utopia,* 1974). Steinberger is a political scientist whose central concerns are philosophical (a previous book was on Hegel). At a sufficient level of generality, both books are about the same kind of difficulty, namely, that we seem to have two modes of judgment: *seeing-that* (immediate, subjectively processless intuition) and *reasoning-why* (step-by-step reasoning). But what if the two conflict? Or what if a seemingly compelling direct intuition cannot be backed up by cogent reasoning-why? Or what if what seems like compelling reasoning-why yields a result that seems intuitively intolerable? Both writers are focused on this family of concerns; but there all resemblance ends.

Steinberger's book is one long argument, mostly in the form of a commentary on famous earlier writers, from Plato and Aristotle, through Hobbes and Kant, to Oakeshott and Arendt in our own time, all on the fundamental theme of the intuitive sense of things that yields good political judgment and the need to tie that to careful reasoning-why—in particular, so that we have a chance to notice, discuss, and perhaps step beyond the absolute thrall of the prejudgments (*prejudices,* as Steinberger frankly labels them) that facilitate but also inevitably entrap our intuitions.

All this seems entirely reasonable. But it was hard for me, as an outsider to the mode of discourse used here, to see what emerged from the discussion that was substantially different from my impressions before starting the book. An oddity of Steinberger's argument seems to be the lack of any reference to Max Weber's famous essay, "Politics as a Vocation," which has a lot to say on the character of good judgment in politics and how it differs from good judgment in more private realms. On the other hand, I expect that readers with a taste for political theory in the first sense may find a good deal more that is controversial and insightful in Steinberger's analysis than I have managed to appreciate.

When we come to Nozick, things could not be more different. There is a thread of a common style through the book, but not of a single sustained argument. Rather, the book is something of an anthology of puzzles of rational choice that have caught Nozick's attention, which he then offers his ideas on resolving. The resolutions are cleverly presented; but they usually have a mechanical and unconvincing character, like fixing up difficulty in Ptolemaic astronomy by adding more epicycles. They seem to me to paper over, rather than resolve, the difficulties of rational analysis of human judgment.

The most central of Nozick's puzzles concerns rational belief where there is a conflict between what rational analysis would lead a person to believe and what would do the most good to believe. Nozick uses the example of a mother whose son appears, by usual standards of judgment, to be clearly guilty of a horrible crime: Might it be rational for her to refuse to believe her son in fact is guilty? And (as in past work which has spawned a large literature) Nozick uses "Newcomb's puzzle" as an artifactual exemplar of such dilemmas, where common sense does not seem to want to go along with good reasoning. In stark form, suppose you are the latest in a long line of people confronted with this choice: there are two boxes, one transparent and containing $1,000, the other opaque from the side you see it but transparent from the back. That box contains either $1,000,000 or nothing. A television camera looking at the back has shown everyone in the country (except you) whether that second box is empty or contains the $1,000,000. Your choice is to take both boxes (giving you $1,000 plus whatever, if anything, is in the second box) or to take the second box alone.

The usual rational choice analysis is that you take both. The million is either in the second box or it is not, so how could you lose by taking both? But Newcomb's premise is that after hundreds of such episodes, you know that without exception, each time the chooser took both boxes, the second box turned out to be empty and that without exception, each time the chooser took the second box alone, he found it contained the million dollars.

Nozick thinks (as most readers would and I certainly do) that it may not be irrational for the mother to refuse to believe her son is guilty and that it also would not be irrational for the chooser to take only the opaque box, despite a clear violation of the usual sure-thing axiom of rational choice. So Nozick would like an extension of the notion of rational choice that allows for choices like these to be deemed rational. Nozick's resolution is to make belief a weighted function of what he calls "credibility," which is not precisely defined but is akin to a Bayesian prior: this is the standard rational choice component. But that is to be augmented by an anticipated value component (the value to the mother of leaving open the possibility that her son is innocent, the value to the Newcomb's chooser of probably winning the million) and also a symbolic value (e.g., the psychic cost to the Newcomb chooser of violating rational principles).

All this, though cleverly worked out, does not seem to me to really help. The assessments required will usually be highly vulnerable to the prejudices that worry Steinberger but do not seem to bother Nozick, and the weights given the three motives are left undefined. All in all, it seems a scheme which can rationalize any belief ex post, whereas the great value of careful reasoning and more formalized rational choice analysis is that it helps us confront inconsistencies in our intuitions, (which had been taken for granted) and eventually reach more reliable intuitions. If we lived in a world where Newcomb's problem was actually credible, then (with lots of experience with that) we would find our intuitions about how sure the sure-thing principle is would change, just as physicists eventually found their intuitions about what surely must happen in subatomic processes changing. And the mother's dilemma seems better understood in terms of human sympathy for another human in a terribly painful situation than as an extension of rational choice theory.

On the other hand, though I have mostly complained about first Steinberger's, and then Nozick's, book, it is characteristic of really important problems that fundamentally satisfactory resolutions are hard to come by, sometimes not even possible. In their very different ways, both books reveal the intelligence and seriousness with which the authors have pursued their projects. If very few readers would find both books to their taste or either one wholly convincing, it is probably also the case that few readers would not find at least one a profitable exploration.

University of Chicago HOWARD MARGOLIS

Morality and Politics in Modern Europe: The Harvard Lectures. By Michael Oakeshott. Edited by S. Letwin. New Haven: Yale University Press, 1993. 115p. $22.50.
Religion, Politics and the Moral Life. By Michael Oakeshott. Edited by Timothy Fuller. New Haven: Yale University Press, 1993. 164p. $22.50.

Although Michael Oakeshott lived into his ninetieth year and thus came within a twelvemonth of outliving the Hobbes on whom he had thrown more light than any other twentieth-century interpreter, he published only three books: *Experience and Its Modes* (1933), *Rationalism in Politics* (1962), and *On Human Conduct* (1975). It is therefore unusually important when "new" works by the most eminent "skeptical liberal" since Montaigne and Hume are brought into the light. This is especially true because two of the three published books, wonderful (and wonderfully written) as they are, represent Oakeshott only incompletely. *Experience and Its Modes* is most notable for its brilliant reflections on the philosophy of history (which R. G. Collingwood praised to the skies) but so denigrates mere "practice" as to leave political theory little place: while philosophy aims at "experience without reservation or arrest, without presupposition or postulate, without limit or category," "practice" (including politics), by contrast, is indeed an "arrest" in experience because it one-sidedly interprets the world *sub specie voluntatis,* exhausts itself in the (necessarily doomed) enterprise of converting what is into what "ought" to be: "The presupposition of practical experience is that 'what is here and now' and 'what ought to be' are discrepant. . . . Permanent dissatisfaction is inherent in practical experience; the explicit assertion of reality in practical judgement is never complete and consequently the world of practical experience is . . . abstract and incoherent" (*Experience and Its Modes*).

To be sure, Oakeshott had retreated from that extreme demi-Hegelian position by the time he published *Rationalism in Politics* 30 years later. But one can wonder whether that all too brilliantly successful volume, with its insistence on the "sympathetic pursuit of intimations" within inherited historical "traditions" does not cast a misleading "Burkean" light on Oakeshott's vision of politics; for the most famous essay in *Rationalism*, "Political Education," first annihilates two straw men ("empirical" and "ideological" politics) in a way that reduces one to helpless laughter, then magnifies "our tradition of political behaviour" in language deliberately reminiscent of *Reflections on the Revolution in France* ("Politics is steady because, though it moves, it is never wholly in motion. . . . Authority is diffused between past, present and future"). Following *Rationalism*'s attack on "ideology," such philosophy as survives is more or less identified with the "rationalist" schemes of second-hand Cartesians and third-rate Kantians: "With every step it has taken away from the true sources of its inspiration, the rationalist character has become cruder and more vulgar. What in the seventeenth century was *L'Art de penser* has now become *Your mind and how to use it, a plan by world-famous experts for developing a trained mind at a fraction of the usual cost*." In much of *Rationalism in Politics*, one sees an Oakeshott wonderfully in command of a satirical idiom but also one more purely "English," less philosophical, than elsewhere.

Only *On Human Conduct* (1975), his magnum opus, presents a fully rounded (though less charmingly funny) Oakeshott. The book, like Gaul, is divided into three parts: part 1 is a compressed, dense, difficult account of human agency, of intelligent self-determination and "self-enactment"; part 2 is an account of the "civil condition" or *respublica* (above all, authoritative *lex*) that *cives* are to "subscribe to" in their pursuit of self-chosen individual satisfactions—with politics restricted to maintaining *lex* (rather than indulging in a Baconian transformation of the world through collective "joint enterprise.") Part 3 is called "On the Character of a Modern European State" but really deals with the advent of every significant moral "disposition" that has appeared in Europe, though it does much to distinguish between the state as a *societas* of authoritative rules suscribed to by *cives* in their chosen self-enactments and the state as a *universitas*—an "enterprise association" run by "teleocratic" managers in pursuit of a "substantive common purpose." And in the most beautiful lyric flight in part 3, Oakeshott celebrates "enjoying individuality" with unmatched pages on Augustine, Montaigne, and Cervantes ("a reading of the human condition as an adventure in personal self-disclosure."

The new Oakeshott volume entitled *Morality and Politics in Modern Europe: The Harvard Lectures*, is a kind of early version of *On Human Conduct*, part 3. Beginning with the claim that "the history of modern European morals displays to us two distinct and opposed moral dispositions or moralities: the morality of individualism and the morality of collectivism" (p. 27), Oakeshott goes on to illustrate his sympathy for the first and his aversion to the second with fine accounts of Montaigne, Montesquieu, Kant, and (unexpectedly) Bentham and with more jaundiced readings of Owen, Fourier, and Marx. (For Oakeshott, J. S. Mill is an equivocal figure—a supposed liberal individualist who is finally unmasked as a devotee of perfectionist uniformity: "Mill's plea for diversity is methodological, not substantial: diversity must be allowed only because we cannot yet be certain about what is true and what is false.") If, in the Harvard lectures, Oakeshott's reading of Montaigne is fine but no match for the lovely pages in *On Human Conduct*, Kant and Bentham, by contrast, are viewed more favorably than anywhere else in the Oakeshottian corpus: in Kant's *Rechtslehre* and other political writings, there is "to be found a genuinely philosophical version of the political theory of individualism." His "task" was to give "the experience of individuality" a "metaphysical and ethical context"; and he "performed it more thoroughly, perhaps, than any other writer." And in the Harvard lectures, Bentham is treated not with the amused contempt of Oakeshott's 1932 *Scrutiny* essay ("As a thinker . . . he is negligible") but as a respectable minor contributor to the morality of individualism: "Bentham did not merely wish each man to make his own choices for himself about his own happiness: he believed that the persons he had to do with were, in fact, persons of this kind."

Fine as the 1958 Harvard lectures are (they also contain splendid pages on Montesquieu, Locke, Smith, and Burke and will stand as a monument to their devoted editor, the late Shirley Letwin), they are really a pendant to *On Human Conduct*. Only Kant and Bentham are seen to better advantage in the earlier work.

It is more difficult to characterize the second "new" Oakeshott volume, *Religion, Politics, and the Moral Life*, since this book pulls together very disparate essays stretching from 1925 to 1955 and includes everything from early pieces on Christianity to reviews of Morgenthau and Walter Lippmann. The Christianity studies from the middle and late twenties are fascinating (with extraordinary "asides" on Pascal and Augustine). Perhaps the finest is "The Importance of the Historical Element in Christianity" (1928), which insists that "a religion, in a manner to which a philosophy is not subject, may be proved lacking by being proved inadequate to the [practical] demands of the ordinary life of ordinary people. . . . Religion exists to satisfy no craving for knowledge apart from the knowledge which comes with the mere strength and courage to take life as it is." (Those eloquent lines foreshadow some of Oakeshott's last thoughts on religion, in a letter written shortly before he died to the present reviewer: "I have gone back to 'theology'—or rather, to reflection upon religion. And I would like, more than anything else, to extend those brief pages in *On Human Conduct* into an essay . . . on religion, and particularly on the Christian religion. This ambition came to me, partly, from my re-reading all that St. Augustine wrote—St. Augustine and Montaigne, the two most remarkable men who have ever lived. What I would like to write is a new version, a post-Montaigne, post-Pascal, post-Blake version of Anselm's *Cur deus homo*—in which (amongst very much else) 'salvation', being 'saved', is recognised as [having] nothing whatever to do with the *future*." Those sentences, from 1986, have roots going back to the twenties, as the "new" volume makes very clear.

Among the later pieces in *Religion, Politics, and the*

Moral Life, three may be singled out as especially notable. The most radical is surely the *Scrutiny* essay, "The Claims of Politics"—which serves to recall that Oakeshott always believed in a moderated version of Nietzsche's dictum that "every great age of culture is an age of political decline"; for it was in September 1939, when everyone was rushing to philosophical, as well as to actual, barricades, that Oakeshott had the nerve to say:

> Politics is a highly specialized and abstracted form of communal activity; it is conducted on the surface of the life of a society and except on rare occasions makes remarkably small impression below that surface. . . . It is a function mainly of protection.
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> . . . A limitation of view, which appears so clear and practical, but which amounts to little more than a mental fog, is inseparable from political activity. A mind fixed and callous to all subtle distinctions, emotional and intellectual habits become bogus from repetition and lack of examination, unreal loyalties, delusive aims, false significances are what political action involves. . . .
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> But in every society there are, I believe, some for whom political activity would be a perversion of their genius, a disloyalty to themselves, not because they have little or no part in the promotion of the communal interests of their society, but because their part is one which it is essential that a society shall have performed and which it is difficult if not impossible to combine with political activity. And among them, I believe, are those whose genius and interest lies in literature, in art and in philosophy.

That skepticism about politics' worth also comes out, in a lighter and funnier way, in "The Customer Is Never Wrong" (1955), in which Oakeshott insists (against Walter Lippmann) that politics "are concerned with the cultivation of what from time to time are accepted as the peaceable decencies of conduct among men who do not suffer from the Puritan–Jacobin illusion that in practical affairs there is an attainable condition of things called 'truth' or 'perfection'." By contrast, "liberal democracy" is a "skeptical politics in which 'truth' appears not as the opposite of 'error' but merely as the opposite of 'lies' and in which utterance is largely free because it is recognized not as argument but as conversation." And he adds that "if we want instruction about the theory of liberal–democratic freedom of speech it is not to Milton or to Mill that we should go, but to Montaigne and Hume."

Perhaps the most extraordinary essay in this second "new" volume, however, is the one called "The Concept of a Philosophy of Politics" (c. 1946), which is a reworking of the magisterial 1938 article, "The Concept of a Philosophical Jurisprudence." In the postwar reworking, Oakeshott omits the earlier commentary on assorted jurisprudes of the early twentieth century (above all Pound), and preserves his clearest statement of what philosophy can hope to achieve:

> Philosophical thought and knowledge is simply thought and knowledge without reservation or presupposition. . . . It should be thought of as something which happens at the end, when the concepts of science, or common-sense, or practical life are subjected to the revolutionary and dissolving criticism of being related to a universal context.
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> The starting place in philosophy is not some remote region of experience known only to the philosopher. . . . A philosophy of law, for example, does not begin with already defined and accepted abstract ideas such as the ideas of Right, Duty and Obligation. . . . It begins with any concept of law a man, however uninstructed, may happen to find in his head. In philosophy, therefore, there is no such thing as a transition from mere ignorance to complete knowledge; the process is always one of coming to know more fully and more clearly what is in some sense already known.

And in a closely related (unpublished) piece from circa 1946–50 called simply "Political Philosophy," Oakeshott urges that "the attitude of radical reflective subversiveness" ("without hindrance or arrest") comes out most notably in Plato's *Republic*, Hobbes's *Leviathan*, Spinoza's *Ethics*, and Hegel's *Philosophy of Right*—those more-than-ideological works that "mediate between politics and eternity." (In connection with the "radical subversiveness" of Hobbes, it is a pity that room could not be found, in this second "new" volume, to republish Oakeshott's wonderful 1935 Hobbes essay from *Scrutiny*: "Radicalism, extravagance, the intrepid following out of a theory conceived in the grand manner and the absence of any sign of alarm, dismay or compromise are not qualities often to be found in English thinkers; but they flourish in Hobbes almost unchecked. . . . Hobbes appears never to have been even tempted to make his conclusions more moderate.")

It remains only to say that Timothy Fuller, by retrieving these long lost or never-published works of Oakeshott, continues to place us deeply in his debt. More than any other scholar, Fuller has helped to preserve Oakeshott's poetic voice "in the conversation of mankind."

University of Wisconsin, Madison PATRICK RILEY

Pluralism: Against the Demand for Consensus. By Nicholas Rescher. New York: Oxford University Press, 1993. 208p. $29.95.

The subtitle of Nicholas Rescher's new book explains it all. It is less concerned with defending pluralism than with attacking "the demand for consensus." Rescher, at the start, defines consensus as "uniformity of belief and evaluation" (p. 1)—a fiercer conception of it than many of us would have in mind—and goes on to suggest that consensus has been the aim of many philosophers and social theorists from Aristotle to C. S. Pierce and Jürgen Habermas.

They were mistaken, in his view, (1) because the pursuit of truth does not necessarily produce consensus and it is truth, not agreement, that the philosopher should be concerned with and (2) because, since "value dissensus inheres in the human condition" and "value disagreement is . . . ineliminable," it is simply "unrealistic and unreasonable" to try to achieve consensus in such matters (pp. 131–32, 134). In fact (not surprisingly), Rescher sees the pursuit of consensus as politically sinister, "the last stand in an ethos of democracy of a pre-democratic *dirigisme*" (p. 3). In the final chapter, he tells us that the aim of political consensus derives "from a fundamentally socialistic commitment to the co-ordination and alignment of individual action into the uniform social order of 'rationalized' central planning" (p. 187).

This being the work of a contemporary academic philosopher, the book comes garnished with tables and a good deal of the "If P, then . . ." type of formulation that seems to be obligatory these days, but actually adds little or nothing to the main text. A more serious defect is the almost complete absence of any sense of history or sociology. If this were a work of pure philosophy, that might not matter. But Rescher is arguing that societies do not need consensus and ought not to aim at it, and the first of these contentions is an empirical proposition (at least in part) and needs to be supported by a consideration of relevant evidence. Instead of evidence, we get bald assertion. "A viable society can dispense with consensus (the USA is a pretty good example)," he tells us in his breezy way (p. 167). To this outsider it sometimes seems as if the United States, with its frightening levels of urban violence, rigid ghettoization, ethnic conflicts, and sporadic rioting, is a society where the dissensus Rescher celebrates is in danger of generating wholesale fragmentation.

Rescher says that what is needed is not greater consensus but "a mechanism for the limitation of conflicts" (p. 182). But since some of these conflicts spring precisely out of the absence of consensus and since they often produce violence, destruction, and death, what mechanisms are there (beyond the law and law enforcement agencies) that can limit such conflicts? There are currently, in Western societies, groups that are so convinced of their religious duty to try and murder a supposedly blasphemous novelist that he has to live in hiding and under constant police protection. Is that acceptable dissensus? Would it not be better if there was a societywide consensus that regarded murder as intolerable, however scandalous the victim's crime might be?

The real questions in this area are, Does any society need *some* common values in order to function at all? And Are there limits to the range of beliefs and commitments (dissensus) that a society can accommodate without disintegrating? The tendency of Rescher's arguments seems to be toward answering *no* to both questions. But such is the lack of detail and examples in his discussion that it is impossible to be sure.

He seems to envisage a society of coolly rational individuals who find it easy to agree to differ. Thus, he tells us blithely that "only for the narrow-mindedly intolerant will disagreement issue in conflict or diversity of opinion in social friction" (p. 182). Really? How does this apply to racism, for example, or sexism? The trouble is that there are, by Rescher's standards, a great many narrow-mindedly intolerant people about, and dealing with the conflicts they give rise to is a far more intractable matter than he seems to appreciate.

Of course, he is right to argue that modern societies are inherently pluralistic and require a high degree of mutual tolerance. But he does not seriously consider whether there are necessarily limits to the extent of dissensus that any harmonious and peaceful society can, in practice, contain. A mere celebration of the virtues and advantages of disagreement is not enough.

University of Sheffield ANTHONY ARBLASTER

The Foundations of American Citizenship: Liberalism, the Constitution, and Civic Virtue. By Richard C. Sinopoli. New York: Oxford University Press, 1992. 215p. $32.50.

At the beginning of this challenging study, Richard Sinopoli declares that its theme is a problem posed by Rousseau—how individuals are to balance the competing demands of civic duties with private desires. He never entirely loses sight of that question, but it soon develops that most of the work is addressed to a far more specific matter, namely, whether, in the debates over the ratification of the American constitution, proponents and opponents were guided by the neoclassical tradition of civic humanism (that man is a political being who finds his highest fulfillment through active participation in public life as a citizen of a republic) or by classical liberalism, either in its "possessive individualist" version or in a more moderate form. Interwoven in that analysis, in turn, is an examination of the philosophical and psychological sources of allegience and obedience, as seen by Federalists and anti-Federalists and the writers who influenced them.

That is an undertaking of vast proportions; and in considerable measure, Sinopoli handles it well. He prudently rejects the more extreme readings of both civic humanism (J. G. A. Pocock, Gordon Wood) and liberalism (C. B. Macpherson) on the ground that they are not much in evidence in the writings of Publius and his leading opponents. Instead, he formulates a reasonable "weak version of the republican thesis" (p. 11) and a version of liberalism that is compatible with a sense of community (as in fact it was in Locke and in the concept as it was understood in America). Then comes the heart of the work in the form of careful, cautious, and thorough analyses of Locke, the Scottish Enlightenment (Hutcheson, Reid, Hume, and Smith), the Federalists, and the anti-Federalists. The result is to restore Lockean liberalism and compact theory to the central place in the American founding. Along the way, Sinopoli clarifies, by subtle reasoning, a number of points that previous scholars had muddled.

But his performance is far from flawless. First, there are some major errors of omission. There is nothing here of the English country-party ideology (and only passing reference to the rediscoverer of that ideology, Bernard Bailyn), without which the anti-Federalists are incomprehensible. The analysis of Hume is satisfactory as far as it goes, but there is no mention of Hume's insistence in *Essays: Moral, Political, and Literary* (1741) that the "stated forms and methods" governing administration are crucially important as a "check on the natural depravity of mankind"—a conception that Hamilton took to heart and made the centerpiece of his statecraft. (Sinopoli does touch upon Hamilton's concern with administration, but he misreads it entirely; indeed, his interpretation of Hamilton throughout is clichéd and uninformed by Hamilton scholarship during the past three decades.)

Errors of commission abound. Sinopoli quotes Madison accurately to the effect that the purpose of government was to secure liberty and happiness but quite misleadingly defines happiness as "the prosperity brought about by industry" (p. 19). Shortly thereafter, he says that "the dividing line between persons with rights and obligations in the liberal policy and 'nonpersons'" was usually "property qualifications" (p. 22). Has he not heard of race, sex, and nonage? He describes Hamilton's reference to "first principles" in *Federalist* 31 as "Lockean" and "Reidian," whereas in reality Hamilton is describing the enthymeme in Aristotelian rhetoric (p. 70). Sinopoli equates Madison's defense of property rights with "liberal capitalist competition," whereas Madison's actual conception of property rights was fundamentally precapitalist (esp. p. 91). He cites Madison's opposition, in the federal convention, to a proposal that the states pay their senators, on the ground that it "would make the Senate like Congress" and then goes on to explain, "that is, like the House," when Madison was obviously referring to the Congress under the Articles of Confederation.

Finally, there is an act of commission that is utterly outrageous. Instead of the traditional use of the masculine as the neutral pronoun, Sinopoli (or his publisher) regularly alternates between masculine and feminine. The usage is, in most instances, merely silly. But sometimes it is an unconscionable distortion of history—as when, in discussing Pocock's definition of classical civic humanism, Sinopoli writes that "any individual with insight into her own well-being would seek the development, or perfection, of her most essentially human faculties" (p. 10). At the very heart of republicanism, in the ancient world as well as in eighteenth-century America, was the understanding that participation in public affairs was for men only. To employ the political correctness gimmick in that context is as inappropriate as in a discussion of abortion (such as is found a few pages later) to speak of a woman's right to control his own body.

University of Alabama FORREST MCDONALD

Psychoanalytic-Marxism: Groundwork. By Eugene Victor Wolfenstein. New York: Guilford, 1993. 468p. $40.00 cloth, $19.95 paper.

This book attempts to evaluate the history of the conflicting relations between psychoanalysis and Marxism, as well as to offer the theoretical bases for a synthesis between the two. After an initial chapter describing the problem to be studied, Wolfenstein proceeds, successively, to establish a genealogy of the relations between both bodies of theory, to lay the grounds of his own vision, and to draw from the latter a set of conclusions for both the understanding of contemporary social struggles and a new emancipatory vision. To accomplish the first of these tasks he starts by contrasting the visions of Marx and Freud. This is followed by a discussion of the various attempts at a synthesis by Wilhelm Reich, Eric Fromm, and Herbert Marcuse, as well as of the contemporary theoretical tendencies that claim to go beyond Freudian Marxism. The second part (which, on top of Marx and Freud, thoroughly discusses Hegelian philosophy) explores the concepts of *consciousness* and *alienation* and gives a central role to the category of *recognition*. This centrality is the result of the pluralization of emancipatory struggles in contemporary society, which requires going beyond a vision of the interaction between groups conceived in purely antagonistic terms: This development is a consequence of the pluralization of emancipatory politics. Classical Marxism, as a theory of class warfare, can bypass the issue. . . . But the world we seek to win cannot be all female, all male, all black, all white, all African, all Asian, all European, etc. At the existential level some particularity is not only unavoidable but vital and vitalizing. Hence the importance of the problem of recognition" (p. 169). Finally, in the last part, Wolfenstein confronts his theoretical perspective with contemporary struggles around class, gender, and race and postulates a project of human emancipation grounded in seven principles: going beyond the systems of alienated labor, making human interests inclusive, removing impediments to emotional growth and well-being, limiting desire to possible human experience, aiming at mutual recognition, replacing hierarchical social organizations by participatory ones, and preserving natural–environmental conditions.

There is no doubt that the problems addressed by Wolfenstein are central ones in contemporary societies. It has also to be said that his recognition of the plural and fragmentary nature of contemporary social actors is a merit of his book, as well as his attempt to go beyond a purely antagonistic notion of social interaction (as was the case with the classical notion of *universal class*). But the shortcomings of Wolfenstein's theoretical enterprise are also only too visible. Perhaps the root of most of them is the entirely unilateral and arbitrary selection of the authors that he has decided to discuss. There would certainly be no objection to a purely analytic exercise, entirely devoted to the internal critique of some classical texts (Marx's and Freud's, in this case). But Wolfenstein does not confine himself to this task. He makes, instead, detailed references to a variety of intellectual traditions derived from those basic theoretical matrices. He does so, however, on the basis of such an arbitrary selection that one cannot avoid the conclusion that his intellectual enterprise is uneasily located in a no man's land between intellectual history and analytical presentation.

Psychoanalytical theory, for one, as Wolfenstein well knows, has advanced a great deal since Freud's days. Now, given Wolfenstein's primary interest in alienation, identification, and the dialectics of desire, one would have expected to find in his book a serious discussion of Lacanian theory, which is centered precisely in those notions and has been extended, over the last decades, to the study of the sociopolitical field. But the only reference to Lacan that we find in the book (apart from two other passing mentions) is the following: "Lacan is far and away the most fallocentric and narcissistic of psychoanalystic theorists. One must defeat him in order to win the struggle for psychoanalysis. But this is not the whole story, not even, perhaps, its principal narrative line. Why, especially outside France, all the attention to the narcissistic Lacan? Why the emulation of his language and style? Why the willing acceptance of his terms for the debate about gender? Is this critique, or is it compliance?" (p. 140). Needless to say, there is no attempt to answer these half-rhetorical questions. The same cavalier treatment is reserved for deconstruction (p. 148–49).

The same problems are to be found in what concerns Marxist categories. The author passes too quickly from Marx analyses to his own critique and reformulations, paying little attention to the various attempts by twentieth-century Marxist authors to engage in similar endeavors. Concepts such as alienation or false consciousness, for instance, have been the subject of intense debates in the 1960s and 1970s, with some Marxist authors making them the cornerstone of their approaches while others repudiated them. Wolfenstein, however, takes the fruitfulness of these categories as unproblematic and makes no reference to those debates. The result is that even his more promising ideas appear rootless and without a precise direction.

I do not want my assessment of this book to appear as entirely negative. The author shows a considerable knowledge of the texts of Hegel, Marx, and Freud, and many of his intuitions are sound, original, and well worth pursuing. But I think that the shortcomings of the book clearly outweigh its merits.

University of Essex ERNESTO LACLAU

AMERICAN POLITICS

Uncertain Warriors: Lyndon Johnson and His Vietnam Advisers. By David M. Barrett. Lawrence: University Press of Kansas, 1993. 279p. $35.00.

Whatever Possessed the President? Academic Experts and Presidential Policy, 1960–1988. By Robert C. Wood. Amherst: University of Massachusetts Press, 1993. 208p. $40.00 cloth, $15.95 paper.

A main frustration in doing presidential scholarship is the difficulty of penetrating the black box of White House decision making. Even when there is available a raft of documentation in the form of reports and memoranda, including decision memoranda on which a president indicates a choice, there is usually the sense that important pieces of the puzzle are obscured. Consequently, studies that make the black box even a bit less mysterious are welcomed. *Uncertain Warriors,* by David M. Barrett, and *Whatever Possessed the President?* by Robert C. Wood in somewhat different ways are polished, rich, and useful additions to the literature on the presidency in this regard.

Both focus on advisory structures and processes. Barrett's book presents three case studies of Lyndon B. Johnson's Vietnam decision making. They are the decisions to escalate in 1965; the debate over options in 1967; and the choices made in the aftermath of the Tet Offensive in 1968, including Johnson's decision not to seek reelection. Wood is concerned with the role of academic advisers, especially social scientists, in presidential politics and decision making from the Kennedy through the Reagan years.

The core purposes animating the two books are similar in some respects but different in others. Both, of course, seek to illuminate the shadowy world of presidential decision making. More specifically, Barrett is interested in verifying or confounding the conventional wisdom about the nature and operations of Johnson advisory system that contributed to the fateful results that were the nation's Vietnam experience. Was it, in fact, "narrow, cohesive, and overwhelmingly hawkish?" (p. 169) Were aides afraid to offer candid advice? Was Johnson close minded about the war and prone to orchestrate agreement among his advisers in support of his predetermined positions? Was it stable, or did it change over time? Was, then, the advisory system simply an extension of Johnson's complex and often difficult personality?

Wood charts the ups and downs of presidential advisers with academic credentials through six presidencies. In addition, woven through the book is a brief. Essentially it is that academic social science, with its "reliance on data, the disciplined use of inference, the articulated distinction between fact and value," (p. 168) can make important contributions to public policy.

A strength shared by the two books lies in clearly stated conceptual frameworks in which analyses are set. Barrett's baseline for his assessment is presidential rationality which is defined as "responding to the environment by developing an advisory system to help determine and reach policy goals, seeking diverse views and information, and connecting means to ends" (p. 9). He also specifies a relationship between the political environment and the characteristics of advisory systems. Wood's core concept is "talk," political talk about attaining and using power. This is the basis for the questions at the heart of his analysis: "Where do the ideas come from . . . Who offers ideas on politics, through what form of talk, and at what time in the political process" (p. 3)? Presidential candidates and presidents mainly talk to three elites: other politicians, professional administrators, and policy experts. There are also outsiders involved in presidential talk, including political philosophers and image makers. Three types of talk, rhetoric, ideology, and knowledge, compete in the presidential arena during campaigns, transitions, and in governing. Decisions arise out of "the ever-present struggle between the ideas of the president and those of the three elites" (p. 6). Wood participated personally in presidential talk for many years starting in John F. Kennedy's presidential campaign, followed by service as chairman of two of Johnson's Great Society task forces, then as under secretary and secretary of the Department of Housing and Urban Development.

Based upon a meticulous examination of the Johnson papers, the papers of other participants, such as Senator Richard Russell, oral histories, memoirs, interviews, and previous studies, Barrett makes a convincing case that Johnson's Vietnam advisory system was rational. Overall and in the particulars, the reality he depicts is far from the pathological horror often described. It was fairly open system, though disguised by Johnson's insistence on confidentiality, and populated by persons in and out of government with diverse views. Johnson listened to and seriously considered the varied points of view and assessments that came to him through a variety of channels.

Moving beyond understanding Johnson's system and how it worked, important in and of itself, of broader interest is Barretts' effort to understand how the system evolved over time. Basically, he suggests, advisory systems are shaped by presidential preferences, but also by the political environment. He theorizes that they will be orderly and collegial when the president's goals and the means to achieve them are clear. They will be less orderly and more competitive when goals and the means to realize them are controversial and unpopular. When goals are clear, but there is uncertainty as to means, they will be a hybrid. He finds the advisory system to be of the third type in 1965, but slightly more competitive than collegial. In the hybrid form of 1967, the collegial outweighed the competitive element in the system. The following year, 1968, although collegiality was still evident, there was a distinctly higher level of competition among Johnson's advisers. His personal style did not change, but his advisory system did.

Barrett concludes his book with consideration of Johnson's political style and the relationship between rationality in advisory processes and the quality of decisions. He concludes that the particulars of Johnson's Vietnam decision were shaped less by the nature of his approach to advice taking than by his world view and the political circumstances of the time.

Wood treats the reader to a lively and interesting history of academic advice based upon his own experiences and systematic investigation of its place in other presidencies. Much of his description of academic advice in post-Johnson presidencies is fresh material. Not surprisingly, he finds that the high water mark reached by academic social scientists and their knowledge talk was during the days of Johnson's Great Society. In subsequent presidencies, although there were academics present, they were most evident in campaigns and transitions, less so in governance. In the Nixon and Carter presidencies, lawyers became increasingly important in shaping policy. Over time, knowledge talk unfortunately has lost ground to rhetoric and ideological talk, Wood believes.

He seems to be somewhat ambivalent about the Reagan presidency, however. On the one hand, for Wood, Reagan was "a man of beliefs, not ideas" (p. 139), and ideological talk was a prominent part of his presidency. The ill-conceived supply-side economics and the "reckless rush to deregulation" (p. 160) were products of ideology, not knowledge. Yet in other areas, such as the New Federalism proposals, Wood sees circumstances that cause him to conclude, "The pattern of advice and counsel from the social sciences not only persisted in the Reagan administration; it was expanded and, in its implementation, perfected," (p. 160). As Wood observes, the line separating ideology and knowledge is blurred, and it remains so in present understandings of the Reagan presidency.

Another troublesome part raised but not fully resolved in Wood's book is, in general, the loss of currency of academic social science and the present preeminence of ideological talk over knowledge talk. In his last chapter, there is a provocative essay that touches upon a variety of possibilities, including the troubled state of universities and social science disciplines that make them inhospitable places for scholars who wish to relate knowledge to policy, the rise of think tanks, an increased analytical capacity within government, and a political environment marked by a resurgent "fervor for capitalism" (p. 174) and by a politics driven by ideological contention. In the end, however, there is no confident explication of the determinants of the quantity and quality of knowledge talk informed by social science in the presidential arena.

In summary, all students of the presidency will be interested in these books. Each makes important contributions to how the personal preferences of presidents mix with institutional characteristics and contextual political forces in the uses of presidential power.

University of Tennessee, Knoxville DAVID M. WELBORN

Agendas and Instability in American Politics. By Frank R. Baumgartner and Bryan D. Jones. Chicago: University of Chicago Press, 1993. 289p. $47.50 cloth, $14.95 paper.

Those who follow public policymaking in the United States long have suspected that the dynamics they observed never seemed to match up to the laws embodied in the discipline's dominant models. Incrementalism reigns, but dramatic and deep changes occur rather often. Policy subsystems are inherently stable, shaped by differential intensity among interests and buttressed by institutional biases; but they erode—or collapse entirely. And of course, the system tends always toward general equilibrium, though many have observed apparent and frequent disequilibria. In short, scholars ask, Which conditions are the norm and which are the exceptions?

Baumgartner and Jones have produced the book that students of public policy have been awaiting and that more than a few will wish they had written. In this rich work, the authors show how long periods of policy incrementalism and spasms of change are part of the same equation of issue definition and institutional bias; how policy subsystems both maintain stability but also can succumb to powerful countervailing forces; and finally, how the search for some general equilibrium probably is fruitless. What exists, instead, is a "punctuated equilibrium" of subsystem stability *and* change. Amid seeming stability, there is continual change. In fact, they note, for a political system long regarded as structurally and politically conservative, "one is struck by the vast policy changes that have occurred in the United States" (p. 235).

The authors come to these conclusions honestly. First, this book is based firmly on, and is heir to, a long line of scholarship going back at least as far as Schattschneider (1960), who, it seems, garners more respect with each passing generation. Its theoretical base is built upon the literatures on problem definition and agenda formation, policy subsystems, and (in a very nuanced application), social choice theory. These traditions are woven together in subtle and sophisticated ways, and the first several chapters alone would make this work a worthwhile read for any core graduate seminar on public policy processes.

Second, the authors back up theory with a methodology that is both commonsense and (even more important) readily reproducible. They start initially from a base of solid longitudinal case studies on issues like nuclear power, pesticides, smoking, and urban policy (among others). These cases, taken individually, show long periods of policy subsystem stability interspersed by sharp spasms of change. Taken together, these cases suggest the impact of broader systemic forces on policy subsystems, since so many of these subsystems encounter trouble for roughly the same reasons at about the same times. The authors build from these cases with empirical analyses of media sources, interest group mobilization, governmental attention and action, and policy outputs (with methods and data sources explained more fully in a useful appendix). The result is a truly comparative, empirically supported analysis of several issues across time—a rare commodity indeed.

No one of the authors' findings may surprise those who follow this literature closely. Issue definition and control over the agenda for debate are critical to policy stability or change. Institutions and institutional design can buttress the policy status quo or can offer opportunities to new voices. Policy entrepreneurs exploit their opportunities to define issues and gain access to sympathetic institutional venues and seek to keep others from doing likewise. The ways in which competing elites and interests seek to involve the "apathetic" in a conflict powerfully affects its outcome. Schattschneider, for one, said pretty much the same things over 30 years ago. What *is* important is how Baumgartner and Jones back up what have become truisms with solid analysis and how they extend the literature into new theoretical directions. The perceptive discussion about the interactions between policy "images" and institutional "venues" alone should keep more than one doctoral student busy for a while.

Perhaps the single most illuminating theoretical finding in this work is what the authors call the *dual mobilization* thesis. The mobilization of new voices and previously excluded interests either can come during a wave of enthusiasm for a particular policy (the cycle of "alarmed discovery" and "euphoria" about problem solution explored by Anthony Downs) or out of opponents' criticism of, and attack against, the policy status quo (as suggested by Schattschneider's notion about the "socialization" of conflict). But each type of mobilization produces a far different outcome. A "Downsian" mobilization is marked by a positive policy "image" (e.g., "peaceful" uses of nuclear power) and by the creation of institutions likely to support subsystem hegemony (e.g., the Atomic Energy Commission). A "Schattschneider" mobilization, by contrast, involves negative policy images (e.g., the health dangers of nuclear power) and the greater policy role played by institutions (e.g., congressional oversight committees) less likely to offer unalloyed support for the status quo. There are, in politics, two very different kinds of mobilization; and which one dominates at any given point explains convincingly whether a subsystem is or is not stable—hence, *punctuated* equilibrium, or stability *and* change.

There is so much offered in this book that any one review may not do it justice, so read it yourself. It is one of the more important books in years, a powerful and well-supported rejoinder to narrowly mechanistic and deterministic policy models. It reminds us that ideas, institutions, and (yes) politics all matter. Its lessons to students of public policy are several: look long (into history), look broadly (beyond the narrow confines of a single institution), look deeply into issues, and compare across issues and across time. It is at once a grand synthesis of the past and a pathbreaking work against which future studies will be measured. In these ways, it is a worthy heir to a long tradition.

Agendas and Instability in American Politics is well written, particularly for a work where the hands and souls of both authors are evident. Any quibbles about the book (a couple of the later chapters fit in a little awkwardly) are minor. This book is recommended especially for graduate courses in American and comparative politics, public policy, and interest groups, among other topics; but it also is accessible for upper-level undergraduate offerings.

Northeastern University CHRISTOPHER J. BOSSO

Careers in City Politics: The Case for Urban Democracy. By Timothy Bledsoe. Pittsburgh: University of Pittsburgh Press, 1993. 231p. $39.95.

Ambition and Beyond: Career Paths of American Politicians. Edited by Shirley Williams and Edward L. Lascher. Berkeley: Institute of Governmental Studies Press, 1993. 260p. $21.95 paper.

The literature on political careers, writes Timothy Prinz in *Ambition and Beyond,* encompasses the fields of "political recruitment, political elites, personality and politics, political biography, legislative studies" (p. 11). But from the books under review, it is clear that as "disparate and far-flung" as this literature is (ibid.), one theoretical approach dominates. Joseph Schlesinger's ambition theory looms large both in Williams and Lascher's collection of essays and in Timothy Bledsoe's study of city politicians. These books offer us an opportunity to see how Schlesinger has influenced our understanding of American political careers.

Ambition theory states that officeholders' ambition for higher office shapes their behavior. Most politicians hold *progressive* ambition (the desire to move to higher office), rather than *static* ambition (to remain in their present office) or *discrete* ambition (to leave office). Schlesinger's work suggests three broad areas of inquiry into differences: ambition, opportunities, and the behavior of politicians in one institution whose sights are set on moving to another. But the "key component," writes Prinz, is the structure of opportunities, or "the seats available and the hierarchy of positions for advancement," which "give shape and definition to the political career" (p. 27).

Although they do not follow a common format, the Williams and Lascher essays fit together well. The first section includes an introduction by Shirley Williams and the literature review and prospectus by Prinz. Williams offers comparisons between political careers in the United States and Britain. Prinz lays out the three stages of political careers discussed in the essays: recruitment, advancement, and retirement. The book includes sections on congressional careers (Linda Fowler and John Hibbing), subnational legislative careers (Peverill Squire and Edward Lascher), and careers for women and minorities (Susan Carroll and Fernando Guerra). The essays are drawn together by Lascher's introduction to each section and his concluding essay summarizing the critiques of ambition theory offered by the other authors.

While some of the research here can be found elsewhere, it is kept fresh by the authors' efforts to use their findings to speak to ambition theory. Hibbing, for example, uses his research on the internal congressional career to show how ambition theory actually misses a great deal of ambitious behavior. Ambition theory tends either to assume that all politicians have progressive ambition or to infer ambition from behavior, that is, that those who run for higher office have progressive ambition. This is limiting, however, because "progressive ambition can just as easily be displayed by running for Party Whip as by running for the Senate" (p. 131). Focusing on a predetermined hierarchy of offices and *office seeking* as an outlet for progressive ambitions misses how ambition may be satisfied by *position seeking* within an institution. The critical question, Hibbing argues, is "why some members vent progressive ambitions by employing the many ladders within the House, while others do so by attempting to jump to a different institution" (p. 131).

Linda Fowler's essay on recruitment demonstrates the need to appreciate how ambitions interact with opportunities and institutional characteristics. A puzzling aspect of recruitment in an era of candidate-centered campaigns, she writes, is "why individuals who could be the strongest candidates . . . never think about running for Congress at all" (p. 89). Restricting ourselves to those who want to move on means we will miss many of the ways politicians satisfy their ambitions. For example, ambition may be more satisfactorily pursued in some local and state governments than in others. Fernando Guerra finds that in the Los Angeles County hierarchy of elective offices, many local offices rank above a seat in the House. We might easily misconstrue the relationship between ambition and career choices by politicians from this area without recognizing both how the hierarchy of offices differs from other areas and the special appeal of retaining a local office. Peverill Squire shows that state legislatures can be set up to offer satisfactory political careers in their own right and that legislators in many states are willing to stay there.

The emphasis in these essays on differences in opportunities across states, localities, and institutions has its payoffs. Guerra, Fowler, and Carroll each demonstrate the value of this emphasis to understanding the career paths of women and minorities. But with the exception of Lascher's contribution on job satisfaction in local governments, we learn little about differences in political ambition and even less on how ambition shapes behavior.

By drawing both on Barber and Schlesinger, Bledsoe offers us greater insights into these two aspects of ambition theory. From a panel survey of over nine hundred city council members interviewed in 1982 and 1987, Bledsoe looks at who seek city council positions and why, how city council careers unfold, and the future offices sought by city council members. Well integrated into the literature on careers and city politics, the analysis is empirically rich, using demographic data, motivations, and council characteristics to explain city political careers. Motivations are measured along four dimensions, the most important of which is from politico to apolitical. *Politicos* are progressively ambitious people who enjoy politics and call themselves politicians.

Bledsoe's research is premised on the belief that city politics need revitalizing. His goal is "to shed light on the means to attract skilled politicians back into city politics" (p. 8). Presenting a counterargument to term-limitation activists, he calls for city governments made up of ambitious, experienced politicians willing to serve long terms. Testing the heart of ambition theory ("the consequences of some people having aspirations for higher office"), he finds that upwardly mobile city councillors had "an increased constituency awareness, an increased constituent attentiveness, and an increased level of energy" (pp. 148, 170). While his data offer a way of looking at how ambition shapes behavior without assuming ambition from subsequent office-seeking behavior, he measures aspirations based on whether council members sought higher office. But at other points he does find interesting differences among motivational types, differences that support the need to bring more career oriented politicians into city politics. To those who would oppose careerist legislators, he responds that "the most that might be feared from lengthy legislative service is the development of political skill and governmental knowledge" (p. 181).

Bledsoe is concerned that city politics are not good places to launch a career for higher office and that most offer unsatisfying careers for their members. In sharp contrast to Hibbing's findings for members of Congress, Bledsoe finds that most people do not change much in how they approach their jobs or constituencies during their time on councils. Further, data he uses from Schlesinger show that city governments were never, in this century, good paths to higher office. Nonetheless, the "solution to political revitalization of cities" is to reform councils so they attract career politicians (p. 175). These include longer terms, the removal of limits on successive terms, and higher salaries. "If city politics can attract strong political talent, then that talent," he asserts, "will in turn excite the people" (ibid.).

Not all of Bledsoe's conclusions are convincing. First, it is not clear what Bledsoe means by "revitalization." The problems with city politics are not concretely identified with reference to any earlier period when politics were somehow better. Further, city councils have rarely been a means to higher office, so it would seem that far more than tinkering would be required to change this pattern. Second, he argues that politicos are better councillors and that we should try to attract more of them. Yet from his data, they do not appear to change much once in office, so it is not clear why we should expect them to develop political skills and knowledge. Finally, he may not give sufficient weight to how the types of issues in city politics attract particular types of people. While he notes Paul Peterson's argument about how city interests constrain policymakers, he argues that this "simply puts a premium on creative and talented political leadership" (p. 17). Yet his own data show a preponderance of business people and preoccupation with the city's economic vitality. Whatever the reforms, it may never be possible to attract the broad spectrum of progressively ambitious people that he seeks in city politics. Indeed, this, more than any other factor, may explain why city councils have never been routes to higher office.

Pennsylvania State University MICHAEL B. BERKMAN

Follow the Leader: Opinion Polls and the Modern Presidents. By Paul Brace and Barbara Hinckley. New York: Basic Books, 1992. 243p. $25.00 cloth.

Media Polls in American Politics. Edited by Thomas E. Mann and Gary R. Orren. Washington: Brookings Institution, 1992. 172p. $28.95 cloth, $10.95 paper.

Is the recent, media-driven proliferation of public opinion polls a blessing or a curse on American democratic politics? Thomas E. Mann and Gary R. Orren, editors of *Media Polls in American Politics*, and Paul Brace and Barbara Hinckley, authors of *Follow the Leader: Opinion Polls and the Modern Presidents*, provide similar answers to this question: media polls can be both blessing and curse, but the present situation often favors the latter. After carefully detailing various problems associated with, or caused by, such polls, both books offer encouraging and sometimes helpful advice on how we (academics, pollsters, journalists, public officials, and citizens alike) can turn the tide and make public opinion polls more valuable and useful to democracy. Media polls are not going to go away, so we might as well make the best of them. The first step, the authors suggest, is for everyone to become more informed and thus more realistic about what the polls mean, what forces influence the polls, and what impact the polls have on American politics.

The Mann and Orren volume brings together polling experts from both academic and news media organizations to discuss the use and misuse of media polls. The editors frame the discussion around two questions: (1) What are the strengths and weaknesses of media polls? and (2) What impact do media polls have on politics? The ensuing debate, which (not surprisingly) finds the academics on the offensive and the media pollsters on the defensive, focuses mainly on the first question. The second question is usually addressed only indirectly. The underlying and occasionally recognized argument in each chapter is that polls foster, rather than subvert, democracy when they accurately and honestly reflect, rather than create or distort, public opinion.

Although relegated to the fourth chapter, Henry E. Brady and Gary R. Orren's essay, "Polling Pitfalls" sets the agenda for the debate. Most other chapters can be read as responses to the powerful critique they offer. Mixing survey research with journalism, Brady and Orren argue, is like trying to mix oil and water. Each has an "ethos" or set of priorities that sets the at cross purposes (p. 56). Journalists' primary mission is to produce novel, interesting, and easy-to-understand stories as quickly and as cheaply as possible. Survey researchers and statisticians, on the other hand, are a very conservative bunch. They must be certain of the validity and reliability of their measures before they will venture even the most qualified conclusion. The result of this "inherent mismatch," the authors assert, is that media pollsters and journalists are not only more likely than academics to commit methodological and conceptual errors in their surveys, they are also more likely to ignore them (ibid.).

Brady and Orren identify and skillfully explain three types of errors found in survey research and especially in media polling: (1) sampling errors, the most troubling of which is low response rates, (2) measurement errors produced by bad questions or "bad arrangement of good questions," and (3) specification errors (p. 59). The last are not the specification errors familiar to econometricians, however. Brady and Orren's specification errors refer to faulty assumptions about the nature of public opinion that affect the kinds of questions asked, how the questions are asked, and how the answers are interpreted. The most common and troublesome of these assumptions is that people have a single, unambiguous, unambivalent and stable opinion on any given public policy issue or public figure. Overall, this discussion of the errors that plague all sorts of survey research provides a very useful methodological guide for survey researchers and users, whether they be journalists, readers, scholars, or students.

The media-based contributors to *Media Polls* are all too aware of the time and monetary constraints that fierce competition places on journalists. Yet in their view, the consequences of these constraints are much more benign than Brady and Orren would have us think. Kathleen A. Frankovic (CBS News), for example, argues that journalists have responded to the demands placed upon them by becoming innovators in survey research techniques. In particular, she attributes the development of exit polling, computer and telephone interviewing technology, tracking polls, panel surveys, and focus groups to media polling organizations. Partisan though she may be, Frankovic provides a useful discussion of advantages and limitations of these polling innovations.

Many of the methodological issues that Brady and Orren consider serious problems, Michael R. Kagay (*New York Times*) considers "legitimate variations in techniques," which fall within "the range of acceptable polling methods" (p. 104). Differences in efforts devoted to increasing response rates, for example, are "subtle differences between equally well conducted polls," not differences in methodological validity (pp. 105–6). Kagay also points out that the competitive forces and constraints to which the news media are subject have fostered the proliferation of national polls. In the pluralistic tradition, he welcomes this development, for it allows us to examine a preponderance of public opinion data on any given topic and frees us from having to rely on the results of just one or two polls.

E. J. Dionne, Jr. (*Washington Post*), on the other hand, acknowledges many of the criticisms against the proliferation of polls. He agrees that instead of helping citizens make informed decisions and fostering democratic deliberation, journalists who rely on polls encourage passivity among their citizen–readers by portraying the political system in an overly simplistic and deterministic fashion. Nevertheless, he claims, these problems are not the fault of media polls themselves. Rather, they are the result of the decline of political parties in the United States and the simultaneous rise of nonpartisan journalism. In effect, the news media have had to assume the responsibility for identifying viable candidates for public office. The polls simply allow the media to fulfill this role in as neutral and apolitical a way as possible.

Most contributors to *Media Polls in American Politics* offer suggestions for making polls more conducive to democratic politics. These recommendations, which are well summarized by the editors, range from the very practical and doable request that journalists fully disclose information about their sampling techniques, response rates, and survey methods to the quite idealistic notion that media pollsters should "familiarize themselves with theories of opinion formation and methods of multivariate analysis" (p. 16). Other recommendations call for journalists to replace their current superficial or nonexistent analysis of polling data with more sophisticated, in-depth analysis. These recommendations, while valid, are somewhat limited. First, they address only media personnel. Do academics, politicians, and ordinary citizens have no responsibility or recourse here? Second, the possibility that the media will take these recommendations seriously seems remote precisely because of the various structural constraints that Brady and Orren argue make survey research and journalism incompatible.

While Mann and Orren approach their topic from the point of view of polling experts, Brace and Hinckley address the democratic implications of the proliferation of opinion polls from the perspective of political institutions. Moreover, *Follow the Leader* provides a more focused and in-depth approach to the larger question by concentrating on one particular political institution (the modern presidency [Truman to Bush]) and one particular aspect of polls (presidential approval ratings). To what extent, Brace and Hinckley ask, do these monthly reports on the presidents' popularity affect presidents' behavior in office, and to what extent do presidents' activities affect their subsequent approval ratings? And what does this say about the strength of democratic linkage between American presidents and citizens?

Brace and Hinckley's careful analysis does give us reason to worry. Concerns about maintaining or boosting presidents' popularity have played a significant role in White House decision making. Presidents do, indeed, appear to undertake certain activities in reaction to recent trends in their approval ratings. Specifically, presidents' major domestic policy addresses and foreign travel are "curiously timed to accord with month-to-month changes in approval ratings" (p. 55). Many of the modern presidents' activities in foreign affairs also appear to be "curiously timed." The use of military force abroad, foreign policy addresses, and international "rally points" (dramatic, or dramatized, events that include—but are not limited to—the use of force) are more likely to occur in the immediate aftermath of negative, dramatic domestic events (e.g., White House scandals) or amid worsening economic conditions. The authors suggest that these foreign policy activities are preemptive strikes taken to prevent a drop in the presidents' approval ratings that would otherwise follow such negative events.

The problem with all this, Brace and Hinckley explain, is not that presidents are concerned about public opinion but that they are concerned about their popularity at the expense of good public policy and real public opinion. Decisions and actions based on what is popular are not necessarily in the best interest of the nation; they may even conflict with what the public actually wants, for presidential popularity as measured in the polls does not tend to reflect the public's policy preferences. Public relations presidents, of which Reagan and Bush are prime examples, are particularly adept at manipulating events to make themselves appear to be concerned about, and in line with, public preferences, when they are, in fact, not. Public opinion in this scenario is not being used by presidents as a guideline for policy decisions, it is being used only for the benefit of the presidents themselves.

Not all modern presidents have been so concerned about being liked. As Brace and Hinckley's analysis demonstrates, Truman, Johnson and Ford were more willing than others to place their convictions, or at least their policy goals, before their popularity. Not surprisingly, these three received the lowest average approval ratings. It is also no coincidence that these unpopular presidents are the very same ones who were not elected to the presidency. Clearly, presidential campaigns and elections—crucial elements of the democratic process itself—foster the public relations mindset.

At the same time that *Follow the Leader* "underscores the worry that the polls turn democracy upside down" (p. 16), it also provides plenty of reassurance that there are limits to what presidents can do about their standings in the polls. In fact, Brace and Hinckley find that most of the activities presidents undertake to boost their approval ratings are futile: they have no discernible effects on subsequent polls—much less, positive effects. Even the use of force will not raise a president's popularity unless the White House puts forth considerable effort to dramatize the event and make it a "rally point." The authors also show that presidential approval is largely a function of economic circumstances and dramatic events that are, for the most part, beyond the control of any individual president. In addition, presidents are set up for failure. They cannot possibly maintain the high approval ratings with which they enter office, for those scores are based on unrealistic expectations that American culture, citizens, and academics place on the presidency. Modern presidents concerned about their popularity may find that the polls control them more than they control the polls. In either case, however, democracy is not well served.

Brace and Hinckley's solution to the problems of the public relations presidency is twofold. First, we should lower our expectations regarding the level of support an incumbent president should receive. A more realistic expectation would be that presidents maintain the same margin of support that elected them in the first place. The White House could then take more policy risks and focus less time and effort on manipulating public information. Second, everyone needs to read this book. Were this to come to pass, presidents would stop wasting their time trying to maintain or increase their popularity, citizens and the media would not hold presidents accountable for events and conditions beyond their control, and (most importantly) Americans would no longer be so easily duped by clever and manipulative presidents and White House staff.

Luckily, most everyone *can* read this book and get something out of it. The analysis is methodologically sophisticated but presented in a very user-friendly manner. For the typical undergraduate or casual reader with little or no knowledge of (much less, interest in) statistics, Brace and Hinckley simply and effectively explain their analysis and present their results in English, in the main text. For the typical graduate student or professor, they explain the statistical procedures and present the results in much greater detail in the appendices. Overall, *Follow the Leader* is a methodological delight. The analysis (either version) is extremely careful and straightforward. The ways in which Brace and Hinckley operationalize concepts and collect data, especially with regard to their vast array of indicators of presidential activities and dramatic events, are remarkably creative and ingenious.

In contrast to the accessibility of *Follow the Leader,* Mann and Orren's *Media Polls in American Politics* is an "insider's" book. Both the general subject matter and the oftentimes technical discussions make it appear to be written for other conscientious journalists and media critics. For the reasons mentioned earlier, the Brady and Orren chapter is exceptional is this respect. Both books, however, address a very important and pertinent set of issues surrounding the proliferation of public opinion polls. Because these issues address the viability of modern-day democratic politics and the linkage between citizens and elected officials, they deserve the attention of said citizens and elected officials, as well as that of scholars and pollsters.

Emory University BETH REINGOLD

Power Failure: New York City Politics and Policy Since 1960. By Charles Brecher and Raymond D. Horton. New York: Oxford University Press, 1993. 393p. $35.00.

A Phoenix in the Ashes: The Rise and Fall of the Koch Coalition in New York City Politics. By John Hull Mollenkopf. Princeton: Princeton University Press, 1992. 285p. $35.00.

These two fine books provide rich, original detail about New York City and contribute to broader issues of urban politics and administration. The Era of Urban Austerity began with the 1975 New York fiscal crisis and continues today. Why and how political leaders then ended some three decades of government growth has sparked much debate. Four answers—two economic and two political—are commonly advanced: (1) capitalism strangled America's most generous local welfare state; (2) decline in jobs and population, exacerbated by growth in the Sunbelt and abroad, devastated the economic base and in turn suppressed government spending; (3) Washington, especially under Presidents Ford, Reagan, and Bush, cut grants, the lifeblood of New York's social welfare programs; (4) finally, there is the regime theory of Clarence Stone and others, which stresses the constraining force of business elites. The interest in these two books is that they review carefully these common views but disagree with all of them. Both books build extensively documented analyses stressing specifically political decisions. They thus extend the subtradition of work stressing political forces, from Ken Auletta and Ester Fuchs, among others.

Mollenkopf's *Phoenix in the Ashes* considers the Koch era. It speaks to the general interpretative issues by showing how dramatically different policies emerged under the same mayor. When Koch first took office in the wake of New York's fiscal crisis, he imposed major cutbacks that his predecessor Abe Beame had considered "impossible." But by 1980, Koch led a major turnaround: New York's spending started growing and continued through the 1980s, despite Republicans in the White House. What Mollenkopf contributes is a detailed interpretation of how and why such policy shifts occurred. His answer stresses changes in the leading groups comprising the Koch coalition. Koch initially ran on a strong antiunion, antibureaucracy platform and appealed to "average citizens," stressing lower taxes and increased productivity. But over the years, Koch moved away from his traditional liberal-left political base in Greenwich Village and upper Manhattan to build a new coalition incorporating Latinos and white Catholics outside Manhattan. With the old Tammany–DeSapio Democratic organization no longer a serious threat, he incorporated many regular Democrats, who, along with certain developers and finance firms, contributed generously to his campaign funds. Most controversial, however, was his increasing exclusion of low-income African-Americans, which eventually led Koch to be labeled a racist and David Dinkins to replace him. Thus, while Mollenkopf recognizes demographic and economic constraints, "astute political entrepreneurs" occupy the driver's seat. There is much discussion of campaign strategies, governing, and specifics of leadership. In many such instances, Mollenkopf thoughtfully raises broader questions of interpretation and assesses them with brief illustrations from other major cities (e.g., Why and how did Boston, Chicago, and Los Angeles go one route, while Houston and Atlanta took another?). These quick comparisons generally bolster the case that Koch as a leader made specific decisions that led New York to pursue its own unique route, largely flowing from the shifts in the groups he brought into his coalition. Mollenkopf still finds room for serious attention to, and empirical detail on, shifts in New York's economy in the postindustrial era—and many details on ethnic voting patterns.

Brecher and Horton argue against the same four common interpretations of urban politics as does Mollenkopf. Their answer is similar to his: political leaders are key. Yet despite this common answer, the two books are quite distinct. Brecher and Horton's *Power Failure* is "a guidebook" to New York City politics and government over three decades. It provides a longer historical purview, more detailed budgetary and fiscal analysis, attention to specifics on service delivery and government productivity, and many suggestions of specific reforms. These are natural outgrowths of the two-decade-long involvement by the two authors in monitoring and assessing New York City government, generating a shelf of publications that have made them leading policy analysts of the city. Their goal here is to update the classic on New York's politics, Wallace Sayre and Herbert Kaufman's *Governing New York City* (1960). They consider that New York City politics is less open and competitive, less pluralistic, than do Sayre and Kaufman. But they offer an essentially pluralist interpretation in that no single elite dominates decisions. Like Dahl, they stress issue-area differences: the teacher's union may dominate much of education, and the Policeman's Benevolent Association could defeat proposals for a civilian review board by civil rights groups and leading politicians, but land use is more dominated by real estate owners and developers (pp. 8–9). Clearly, the average citizen or minor-group leader may count little. Indeed, they stress that bureaucratic leaders in these various issue areas have surrounded themselves with pressure group allies that make political access by others sorely difficult. Many of their reform suggestions concern ways of seriously introducing more general criteria like value for tax money, since attention to such criteria often run counter to entrenched interests. This is clearly a work by two veterans of political wars; dozens of their past battles are distilled here. The level of detail sometimes overwhelms the reader of *Power Failure*; but if 393 pages are weighty, this is still just about half the length of Sayre and Kaufman.

Mollenkopf's focus on Koch and his political coalitions provide an organizing theme that makes his book narrower but more readable. He makes a useful conceptual distinction between campaigning coalitions and governing coalitions, each of which has its partially distinct dynamics. Koch railed against the unions as a first-time candidate but, once in office, grew closer to them. Brecher and Horton's review of five administrations from 1960 to the present leads them to add much more detail. They provide many specifics, from the quality of refuse collection to detailed breakdowns of outlays on psychiatric inpatient care. Such detail about trees—and leaves—occasionally obscures the forest. Still, for students of New York city government, such detail makes this the new definitive "handbook."

In assessing interrelations between economic and political forces, Mollenkopf chooses a wide variety of specific decisions that he briefly summarizes and interprets. There are no detailed "cases," however, as in Dahl or Banfield. Mollenkopf's conclusion is that "political considerations had a far greater impact on the actions of the Koch coalition than any supposed contextual imperative to promote private investment" (p. 161). Through skillful use of extensive, historical data, Mollenkopf traces the sources of Koch's popularity, the strength of his governing coalition, and the roots of his demise. Still, Mollenkopf has some difficulty explaining the demise of the Koch coalition. It is unclear why Koch's efforts to maintain tensions within his opposition suddenly failed. The role of local economic conditions in the Koch coalition also remains obscure. How he specifically chose a prodevelopment agenda once in office and how "growth politics" was distinct in New York City could be more sharply addressed.

For Brecher and Horton, the task is to demonstrate whether shifting political alliances, a changing local economy, or fluctuations in the level of intergovernmental aid are responsible for policy variations. Comparing changes in budget outlays with changes in these three independent variables, they conclude that local political actors were the most important factor in three of their four time periods. The problem here is that they provide little or no discussion of specific political actors and decisions that they undertook to influence policy.

Differences between the two books stem from different goals. Brecher and Horton are fascinated with the Big Apple. While their analysis has wider implications, their main goal is to provide an encyclopedic guide to New York City politics and government, which they do with considerable detail, ingenuity, and success. For reformers, however, Brecher and Horton lament that on the two criteria of responsiveness and efficiency, New York fails again and again. From an electoral process that is neither competitive nor participatory to a budgetary process and a service delivery system that suffer widespread inefficiencies, Brecher and Horton highlight the power failure of New York's government. This contribution is diminished by a dearth of attention to specific political decisions. Even though a policy option may be more efficient, it may have negative distributional repercussions or other politically important implications. *Who* supports *which* criteria of reform are not as central questions as they might be. Consequently, the reasons for lack of support for their reform suggestions are often vague. They seldom mention race; but it is central for Mollenkopf. He is far more concerned with the political costs and benefits of a given policy; and for him, a policy is successful if it generates greater political support. Obviously, one must read both books.

University of Chicago TERRY NICHOLS CLARK

The Phantom Respondents: Opinion Surveys and Political Representation. By John Brehm. Ann Arbor: University of Michigan Press, 1993. 266p. $39.50.

In this slim volume (fully 50 pages of the already modest length comprise technical appendices), John Brehm presents a broad and detailed empirical analysis of the effects of nonresponse on survey results and traces out the profound implications of that analysis.

Brehm first explores the notion of *representativeness* in both its statistical and political senses. (In this connection, I miss a reference to the four papers by William Kruskal and Frederick Mosteller published in the *International Statistical Review* in the late 1970s that thoroughly explore the definitions and usage of the term *representative*.) He argues that polls pervade political life, influencing the allocation of funds, coloring political discourse, affecting political campaigns by both shaping emphasis on issues and influencing contributions (and perhaps even turnout), and supplying data on which academic theories of politics are tested. But more important, he suggests that surveys present a "unique opportunity . . . to compensate for inequities elsewhere in politics" as they bring the voting booth to respondents' front doors or telephones and hence can potentially increase the representativeness of the political system (p. 18). To the extent that there is nonresponse, representativeness in both senses is compromised. Brehm's aims for the book are twofold: he develops and tests a model for survey nonresponse, and he assesses how nonresponse affects scientific research.

But first he analyzes who is missing in the General Social Survey (GSS) and National Election Studies (NES) from 1978 to 1988, interestingly enough using the Current Population Survey, rather than the census, as the gold standard. The finding—that blacks, women, the elderly, the poor, and the less educated are overrepresented in academic surveys and that men, youths, whites, and the wealthy are underrepresented—is initially cheering in the context of surveys providing political access for groups typically underrepresented in more conventional political arenas. But Brehm points out in a later chapter that it is likely to be atypical members of the conventionally underrepresented groups who make up the overrepresentation in surveys—individuals who are more like those in the usually well represented groups.

Brehm's next step is to use data collected by NES, GSS, and the Detroit Area Study on reluctant respondents and nonrespondents to test his model of survey participation. He uses probit analysis to model contactability and capability of being interviewed—the first two stages of the participation process. But the emphasis is on modeling respondent compliance once contact has been made, postulated to be based on four relationships for the potential respondent: relationship to strangers (Will I talk to an interviewer?), relationship to the interviewer (Will I comply with this particular interviewer's request?), relationship to the interview (Does the idea of being interviewed appeal to me from my past experience or current circumstances?) and relationship to self-image (Am I interested in helping out?). After examining each of these relationships in turn, Brehm incorporates them into a structural model of compliance, pointing out that he has, at best, weak indicators of the underlying concepts and that using those indicators requires believing that the reasons given for participating or declining to participate are, indeed, the real reasons for the behavior. (He points out, for example, that "I don't have the time" may be merely a polite excuse. In addition, cognitive psychologists have taught us that individuals do not necessarily have privileged access to the reasons they do the things they do.) Nevertheless, Brehm finds a good fit for his model across several data sets, with the relationship to the interview being the dominant factor determining compliance. Here and later, Brehm argues that the message for survey researchers is to make their interviews more interesting and inviting to respondents in order to bolster response rates.

Brehm goes on to show how nonresponse can bias univariate and multivariate results of surveys and to carry out a Monte Carlo simulation of selection bias. I find this section to be the least carefully explained and the most difficult to understand of all the material in the book. But he makes a strong and telling argument against weighting as the solution for nonresponse. First, members of an underrepresented group who do participate in the survey are likely to be atypical of the group as a whole; unduly weighting their responses will only compound the bias. Further, weighting by likelihood of participation is shown, in the simulations, to decrease the measured strength of relationships. Instead, Brehm advocates correcting for nonresponse, using strategies of modeling the selection process as proposed by Heckman and Achen. He proposes three models of the sample selection process and applies them to four broad classes of models that political scientists often estimate with survey data. Indeed, taking advantage of the fact that political scientists so often use data from NES, he reanalyses earlier studies on political participation, vote choice, candidate evaluation, and policy position, incorporating correction for nonresponse. He finds that the coefficients for most of the models change only slightly, leaving measures of relative importance approximately the same. But coefficients sometimes double upon correction, making for far different substantive conclusions, especially on issues of turnout and information. I find this section fascinating, but am somewhat concerned with the uncritical acceptance, without any comment, of the restrictive and sometimes dubious assumptions necessary for the selection models.

Finally, Brehm simulates what would happen if response rates were to drop considerably from their current levels by comparing results from NES as he successively drops out those who were the most difficult to contact or to convince to participate. His major finding is that if response rates should fall below about 30%, nothing, not even the modeling of nonresponse, could rescue the results of surveys. He closes with the plea to survey organizations to make their surveys interesting to respondents and to collect and disseminate the collateral information on respondents and nonrespondents that would make modeling of selection possible.

Brehm's work is careful and comprehensive. Hence, the lack of reference to *Incomplete Data in Sample Surveys* (1983), the three-volume report of the Panel on Incomplete Data of the Committee on National Statistics of the National Research Council surprised me. Brehm's volume provides a great deal of important analysis for survey researchers and political scientists to think about and to act upon.

State University of New York, Stony Brook JUDITH M. TANUR

American Political Cultures. By Richard J. Ellis. New York: Oxford University Press, 1993. 251p. $45.00.

Do Americans possess a single political culture? Do they share the same basic beliefs about such vital political ideas as liberty, equality, property, and democracy? According to Richard Ellis, there is a tradition of thinkers, most notably Louis Hartz, who have answered these questions in the affirmative. Their various accounts, he tells us, have tended to focus on the pervasive "individualistic character of the American people" (p. 3). In contrast to this singular and rather ambiguous characterization, Ellis stresses the plurality of political cultures and the multiple meanings of individualism in America.

Drawing from the anthropologist, Mary Douglas, Ellis begins by distinguishing between two dimensions of individuation. Individual autonomy may be constrained either through what Douglas calls group or grid. "Group restrictions on individual autonomy can be measured by determining whether rules of admission to a group are strong or weak, and whether the elite support a group gives to its members is complete or partial" (p. 3). Grid restrictions, on the other hand, concern social regulations or external prescriptions. "At the high end of grid, interaction among individuals is strictly regulated and all members know their station" (p. 3).

Using a simple matrix, Ellis adduces four types of social life: competitive individualism, egalitarianism or communitarianism, atomized subordination or fatalism, and hierarchical collectivism. The following table charts his fourfold typology:

	Low Group	High Group
High Grid	Atomization or Fatalism	Hierarchical Collectivism
Low Grid	Competitive Individualism	Egalitarianism or Communitarianism

Employing this typology, Ellis analyzes the history of the American experience. Against the view that American political culture is predicated on competitive individualism alone, he emphasizes the egalitarian or communitarian form of political culture. "Egalitarianism differs from individualism," he tells us, "in its effort to integrate the individual into a caring collectivity . . . Competition is revered by individualists, solidarity is privileged by egalitarians . . . Participation is the guiding star for egalitarians; self-regulation is the ideal of individualists" (p. 5). As examples of egalitarian or communitarian ways of life, Ellis analyzes the orientations to liberty, equality, property, and democracy proclaimed by proponents of such diverse movements as Puritanism, classical republicanism, and abolitionism.

Although much of Ellis' book is dedicated to examining the rival positions held by competitive individualists and communitarians, he dedicates a chapter each to the political cultures of hierarchy, fatalism, and what he calls hermitude. In his chapter on hierarchy, he binds together Virginia planters and New England Federalists. Employing his typology, Ellis suggests that the former would score to the right on the grid axis while the latter would score to the right on the group axis. In other words, Virginia planters tended to be more stratified and rank-conscious than the New Englanders while New England Federalists tended to be more conscious of the collectivity than their hierarchical counterparts in Virginia.

In his chapter on the various political and cultural orientations and responses to slavery in America, Ellis elaborates the idea of fatalism. "Fatalism," he writes, "is a learned response to a social environment in which there is only a tenuous connection between preferences and outcomes" (p. 135). According to Ellis, such a social environment is depicted by such scholars as Kenneth Stampp (*The Peculiar Institution* 1956) and Stanley Elkins (*Slavery* 1959), recounted in the Uncle Remus stories of Br'er Rabbit, and retold in the slave narratives of William Wells Brown and Frederick Douglass.

Finally, going beyond his fourfold typology, Ellis examines the life of the hermit, exemplified by Thoreau at Walden Pond, who journeys beyond (or at least outside) the horizons of both group integration and social regulation.

In his concluding remarks, Ellis reviews the two major theses of his work. First, he claims that his stress on rival political cultures contradicts the consensus theory of political culture in America that points to competitive individualism as the sole vision of the good life. Given that the scholarship on American political culture over the past 25 years has stressed the debates between such rivals conceptions as republicanism and liberalism, Ellis' contradiction of the consensus theory seems somewhat banal. Indeed, he even admits that this aspect of his book "will seem passe" (p. 152). In fact, a good portion of his work is nothing more than a surface remapping of the already well articulated terrain marked between communitarians and individualists by such notable scholars as Hannah Arendt, Sheldon Wolin, John Homer (Jack) Schaar, Benjamin Barber, and Wilson Carey McWilliams. Well before Ellis, these thinkers clearly demonstrated the force of the communitarian challenge against the pervasive individualism of American life.

Ellis' second thesis concerns his use of a general theory and typology to stake out a middle ground between those who want to universalize a single notion of culture and those who believe that "every distinct time and place and race is endowed with a unique culture" (p. 156). Once again, there is nothing extraordinary about Ellis' thesis. Almost every thoughtful observer of political culture has staked out this same theoretical ground. Indeed, over a century and a half ago, Alexis de Tocqueville balanced a sensitivity to historical and social context with a general theory regarding the prospects of democracy. In Tocqueville's seminal work, each of Ellis' rival political cultures can be easily located. Tocqueville stressed communitarianism in his analysis of the New England township system; he analyzed competitive individualism in his remarks on "self-interest rightly understood"; he warned against social atomization and fatalism in his discussions of individualism and modern despotism; and he associated hierarchical views with both the burgeoning manufacturing aristocracy in New England and the established plantation economy in the South.

To say that there is little new in Ellis' book is not to say that it is without merit. His typology links diverse political traditions, historical movements, and cultural patterns of action together in often very insightful ways. In the end, it provides a useful and clear map of a road well traveled by those who have tried to come to terms with the complexity of American political life.

Kenyon College MICHAEL BRINT

Bargaining with the State. By Richard A. Epstein. Princeton: Princeton University Press, 1993. 322p. $35.00.

As most students of American politics and public policy will readily admit, the power of the federal government is constrained only in the most minimal sense by the Constitution. The emergence of a dense network of government programs providing benefits to citizens has created new means of exerting governmental power. The federal government often provides contracts, grants, licenses, and tax exemptions that increase the welfare of its citizens. However, access to these benefits may depend on the willingness of citizens to abide by specific rules and regulations. This, in turn, creates a means by which the state may exercise powers beyond established constitutional limits, particularly if citizens have but a limited capacity to bargain. Can the government make acceptance of public funding for the arts contingent on the subject matter addressed by the artist? Does the provision of a tax exemption to a religious organization allow the state to intervene in its religious practices should they prove discriminatory. Can the government provide employment only if individuals agree to waive their rights against unreasonable search-and-seizure? Questions of this type are at the heart of Richard A. Epstein's *Bargaining with the State,* in which he seeks to "explore the extent to which, as a matter of political and constitutional theory, it is proper to impose limits on the power of the state to bargain with its citizens" (p. 3).

Bargaining with the State begins with a comprehensive examination of governmental coercion and unconstitutional conditions. In part 1, Epstein seeks to explore the "bargaining risks" associated with government action and to determine what kinds of conditions the state can force on its citizens as a requirement for access to particular benefits. Epstein begins with the doctrine of *unconstitutional conditions,* that is, that "even if a state has absolute discretion to grant or deny any individual a privilege or benefit, it cannot grant the privilege subject to conditions that improperly 'coerce', 'pressure', or 'induce' the waiver of that person's constitutional rights" (p. 5). However, his goal is to refine this doctrine to determine how one might distinguish between legitimate and illegitimate demands.

Epstein begins by accepting that the proposition that some forms of property are best held in common and that the state has a legitimate role to play in managing particular resources. The best situation is to provide for "that initial assignment of rights which allows subsequent adjustments to be made with the lowest level of friction and drag" (p. 39). While a system of private property rights and freedom of contract works well for most situations, one must exert great care when addressing the use of public property because of the monopoly power of the state. With respect to public policy, the key questions are two: "First, what techniques help insure the maximization of the social surplus from forced exchanges initiated by the government? Second, what rules determine each person's share of that surplus?" (p. 19). On the first question, Epstein merely suggests that although each situation must be examined individually, the answer will be tied to the need to minimize transaction costs. It is the second question that receives the greatest attention. Epstein argues that at bare minimum, a just compensation requirement is necessary, assuring that no one is worse off following the exercise of state power. However, the Pareto condition, when taken by itself, "is underinclusive because it leads to the conclusion that the surplus generated . . . may be allocated through political action in any way whatsoever" (p. 93). Epstein suggests that the best rule is to require a maximum improvement in social welfare and a pro rata division of the gain from government action, allowing subsequent private transactions, rather than public policies to redistribute incomes. The concern is that the state may often force individuals or groups to accept situations that are a Pareto improvement over the original state but suboptimal because they are tied to efforts to redistribute and administer social gains. Epstein thus concludes that we should adopt the best achievable state of affairs to judge policy, rather than the status quo ante, if our goal is to maximize the cooperative surplus from government action. As one might expect, one finds little support for redistributional schemes in this framework even if there is a surprising level of support for positive government action.

Epstein's key contribution is developed in part 1 of *Bargaining with the State.* It is careful and relatively balanced in its examination of collective action problems and the problems associated with the state's exercise of monopoly power. The remainder of the book is devoted to an examination of a wide range of contexts in which the bargaining risks are present and questions of unconstitutional conditions emerge. This exploration leads Epstein through incorporation, discriminatory taxation, federalism, public roads and highways, land use restrictions, licenses and permits, employment contracts, tax exemptions, unemployment compensation, welfare benefits, and educational benefits. While Epstein's analysis of some of these topics (e.g., access to interstate highways, the provision of licenses) is only mildly interesting, the analysis of other topics (e.g., Medicaid funding of abortions, the provision of public monies for the arts, and tax exemptions for religious institutions) is truly fascinating. Despite the quality of these chapters, the book would benefit from a greater interplay between the theoretical concerns raised in part 1 and the policies and court decisions examined in the remainder of the book. The theoretical framework is so rich in its implications, yet much of it remains muted or absent from the 11 chapters that follow. Indeed, one can read part 1 and the remainder of the book as largely separate enterprises. A concluding chapter critically reviewing the theoretical discussion of part 1 in light of the subsequent cases would have been a partial solution to the lack of integration. Alas, this is not done. The book ends abruptly, with no more than a few paragraphs devoted to bringing the reader full circle.

In the final chapter, Epstein reminds the reader that "the power to contract and to grant, when lodged in the hands of government, may well prove to be as dangerous as the power to take and to regulate." Although analysts rarely subject government contracting to extensive scrutiny, Epstein is most certainly correct when he notes that "government and statecraft are always a high-risk enterprise, and bargaining by the state has to be watched as closely and with the same level of concern and suspicion, as taking, regulation, and taxation—the traditional forms of government power" (p. 312). In casting new light on this nebulous world of contracts, grants, subsidies, and tax exemptions, Epstein's book expands his audience's understanding of some of the tensions inherent in the modern welfare state. Despite the limitations noted, the early chapters of *Bargaining with the State* provide more insights than one commonly finds in a single volume. As such, this book should be read widely by policy analysts and students of the American political and constitutional system. Its insights are important, even if they remain somewhat underdeveloped.

Wesleyan University MARC EISNER

Candidates, Congress, and the American Democracy. By Linda L. Fowler. Ann Arbor: University of Michigan Press, 1994. 240p. $42.50 cloth, $16.95 paper.

The stated goal of the University of Michigan Press's Analytical Perspectives on Politics Series is to publish works of a synthetic nature that provide "clear and discriminating descriptions of the current state of the art and a strong-minded prescription and structure for future work in the field." The latest contribution to the series is Linda Fowler's study of candidates for Congress. I stress the raison d'être of the series to help prospective readers understand what this book does and does not provide. It is not intended to offer original research if that means new data and/or new analyses of old data. It *is* intended to provide both a creative synthesis of existing research and a strong-minded prescription for future research. In terms of synthesis, I would assign the book a grade of A+, nor would I have to agonize in doing so. In terms of prescription, I would probably assign a B− or C+.

The book reflects Fowler's impressive command of a large literature, as well as her thoughtfulness and insight. Readers would be hard pressed to spend time with the book without learning something. Its organization is driven by Fowler's belief that "the same fundamental question about who ends up on the congressional ballot" is at the core of a wide variety of debates within the discipline (p. 12). As such, she is not much taken with Downsian, party-based views of American politics, preferring instead to stress the candidate-centered nature of the modern era. She is not much taken with rational choice assumptions about goal-seeking behavior, preferring instead to stress the idiosyncrasies of individual candidates. Nor is she much taken with voter-driven models of the polity, preferring instead to see voters responding to, rather than creating, options presented by elite candidates. From Fowler's perspective, candidates are in the middle of not just elections but also representation and institutional change; and chapters on these three topics anchor the book.

As a synthesis, Fowler's work succeeds wonderfully, and this is not surprising since I can think of no one more qualified to survey the literature on congressional candidacy than Linda Fowler. This is not just a literature review but (as the series objective promises) a "creative and sophisticated" synthesis that actually suggests new ways of looking at the literature and new implications of that literature. I found many provocative insights in this engaging, broad-based review. She points out that the challenger quality literature arose as an attempt to explain how national economic conditions and other aggregate forces affect vote outcomes through strategic choices of candidates but that recently this literature has looked more toward local conditions than toward national forces as causes of candidate decisions. She points out that many studies of ambition are, in fact, studies of the opportunity structure masquerading as studies of ambition. And she points out that while institutional change could be due to the desires of members or to changes in the larger environment, neither of these explanations works well empirically.

But offering strong-minded prescriptions for future work is a much more difficult task than synthesis and it is this difficulty, rather than any failing on Fowler's part, that in all probability accounts for the lower mark on this component of the book. The prescriptions offered are sometimes difficult to find. The primary general pleas seem to be for more attention to challengers, as opposed to incumbents (reasonable, but not too specific), and for the explicit realization that congressional candidacy can be conceptualized as both an independent and a dependent variable (already done, though perhaps only implicitly).

A few of the more specific prescriptions may not be directly on target. For example, Fowler believes that one consequence of the discipline's alleged preoccupation with incumbents "is an inability to identify the pool of prospective candidates" (p. vi). I fully agree that this inability is at the heart of dissatisfaction with the study of congressional candidates (it keeps us from doing more by way of comparing candidates and noncandidates), but the reason the pool has not been satisfactorily demarcated is that it is very hard to do—even without a preoccupation on incumbents. Similarly, Fowler does a splendid job of discussing the methodological problems inherent when candidacy is both an independent and a dependent variable, and a few vague references are made to two-stage least squares estimation procedures; but the real problem is obtaining a satisfactory instrumental variable that would then permit estimation. To date, none appears to be available. I applaud Fowler's ability to identify the problem areas so clearly and her proddings that we realize the complexity of the situation, but I worry that realization alone is insufficient for research to advance very far. The difficulty, in my opinion, is not a bias in the research agenda so much as a set of seemingly intractable problems.

Before closing, I should note that recent events could, in the eyes of some observers, leave the larger premise of the book open to question. The surprising resurgence of party voting in Congress, the policy constraints imposed by severe budgetary pressures, and the somewhat formulaic nature of modern congressional service could be seen as conspiring to reduce the importance of individual members of Congress, once in Congress, even as individual candidates for congressional office become more central to election outcomes. Why is it that the massive class of 1992, despite all the expectations that it would dramatically alter the face of the institution, was a dud? Efforts at institutional reform in the 103d Congress came to very little and were not led by the scores of self-proclaimed antipoliticians elected in 1992. One explanation could be that the process of institutionalization diminishes the autonomy and uniqueness of individual members because the socialization process and other forces turn them into company men and women. This is institutionalization in the public administration (rather than the Polsby) sense, so it is seldom employed by Congress scholars; but it represents a valid way of looking at the issue. When it is applied, term limits become a logical step in the evolution of the institution. If individual members do not matter that much because the institution has negated most of their uniqueness, just rotate interchangeable people in and out.

Of course, if the truth be known, I *do* think individual members matter; but it could be that in addition to working a little harder at offering prescriptions, Fowler could defend the importance of individuals as members and not just as candidates. I am not sure this importance is quite as obvious as she believes. But these are mere interpretational quibbles and are generated by the stimulating nature of the book. They are by no means serious problems that should keep people from buying, reading, and assigning the book. It constitutes an erudite summation and critique of the literature on congressional candidacy—a sterling contribution worthy of much attention.

University of Nebraska, Lincoln JOHN R. HIBBING

The Constitution Besieged: The Rise and Demise of Lochner Era Police Powers Jurisprudence. By Howard Gillman. Durham: Duke University Press, 1993. 317p. $34.95 cloth.

Conscience and the Constitution: History, Theory, and Law of the Reconstruction Amendments. By David A. J. Richards. Princeton: Princeton University Press, 1993. 295p. $35.00 cloth.

The role of the judiciary and legal ideology in the development of political institutions is a central concern of the public law scholarship; and much of the research is interdisciplinary in combining law, history, philosophy, and political science. That concern is addressed in the books by Howard Gillman and David A. J. Richards, though in different ways and for very different purposes. Both works are also interdisciplinary in their approaches.

The Constitution Besieged reexamines the rise, in the late nineteenth century, of the Supreme Court's jurisprudence protecting economic rights under the Fourteenth Amendment's due process clause, as well as its eventual demise in 1937 amid the battle over Franklin D. Roosevelt's "Court-packing" plan and the Court's "switch-in-time-that-saved-nine." That period became known as "the *Lochner* era" because of its identification with the jurisprudence of a "liberty of contract" espoused in *Lochner* v. *New York* (1905), which struck down a state law setting limits on the number of hours bakers could work. Gillman, a political scientist, ably advances a revisionist interpretation of that era's jurisprudence.

The standard interpretation of the *Lochner* era takes its cue from one of the dissenting opinions in that case. Dissenting Justice Oliver Wendell Holmes brought the insights of American legal realism to bear on the majority's ruling in *Lochner*. His opinion embodies the principal liberal and progressive criticisms of the *Lochner* era. The majority was *making*, rather than *interpreting*, the law in its creation and enforcement of a substantive, yet unenumerated, "liberty of contract." The Court second-guessed legislatures and Congress, thereby becoming a "super legislature." In *Lochner* and other cases leading up to the 1937 constitutional crisis, the Court ostensibly imposed its own conservative laissez-faire economic philosophy on the country. Substantive due process analysis and "judicial activism" were discredited by Justice Holmes. And they remain discredited in circles of liberal legalism, at least insofar as they apply to the judiciary's defense of economic interests.

Largely overlooked was another dissenting opinion in *Lochner*, filed by Justice John Marshall Harlan and joined by Justices Edward White and William Day. That dissent represents a rival theory of the *Lochner* era's jurisprudence. Justice Harlan considered the "liberty of contract" to be a vital constitutional guarantee, rooted in the Reconstruction Amendments and the antislavery movement's emphasis on self-ownership. However, as he reminded his colleagues, economic liberty remained subject to legislation aimed at promoting public health, safety, and the general welfare. *Lochner*'s bare majority, in his view, mistakenly construed New York's law as a protectionist measure for bakery workers, rather than as a legitimate piece of health and safety legislation promoting the general welfare. And Justice Harlan marshaled empirical studies showing that working long hours in bakeries may indeed be injurious to workers' health. In short, the majority in *Lochner* and its ilk got carried away in standing against protectionist legislation favoring one occupation or group over another.

Lochner's era and jurisprudence thus invites two rival interpretations. On the Holmesian view, *Lochner* represents the Court's purblind imposition of an economic philosophy that ran against the growing forces of progressivism. Alternatively, the *Lochner* era was a time of great class conflict and political struggles during which the Court tried to draw (not always successfully) a principled distinction between legitimate economic legislation promoting health, safety, and the general welfare and legislation that was invalid because it was deemed to advance the special interests of particular groups or classes.

Gillman makes a persuasive argument for the revisionist theory of the *Lochner* era. He does so, first, by examining the origins of police-powers jurisprudence in the early nineteenth century and placing it within the context of James Madison's analysis of factions, as well as the judiciary's enforcement of a standard of legislative neutrality during the Jacksonian period. Gillman then commendably links that analysis with postbellum Supreme Court rulings to establish a continuation of that tradition and legal ideology. Finally, the cases leading up to and following *Lochner*, along with liberal legalism's opposition to them, are analyzed within the framework of political and legal ideological conflict over the "social construction of reality" (p. 16). The bottom line, in Gillman's words, is

> "a story not of the sudden corruption of the law and the judicial function, but rather of how the judiciary's struggle to maintain the coherence and integrity of a constitutional ideology averse to class politics was complicated and ultimately derailed by the maturation of capitalist forms of production. . . . The Crisis in American constitutionalism that we associate with the *Lochner* era was triggered by the judiciary's stubborn attachment to what historical participants perceived to be an increasingly anachronistic jurisprudence, one that had lost its moorings in the storm of industrialization." (pp. 10–11)

The revisionist thesis advanced in *The Constitution Besieged* is not original. Historians, such as Charles McCurdy, David Gold, and William Forbath, among others, have taken a revisionist view of the *Lochner* era's justices, jurisprudence, and judiciary. Yet until Gillman's work, no scholar has developed that thesis in such a sustained and systematic fashion. In doing so, Gillman makes an important contribution to the literature. Nevertheless, readers may not be entirely convinced that Holmes's and liberal legalism's critique of *Lochner* was completely wrongheaded or that Gillman takes fully into account the extent to which a conservative political ideology played a role in the Court's rulings. In other words, Justices Holmes and Harlan each focused on different facets of the same social reality.

By contrast, *Conscience and the Constitution* is a very different sort of book. Richards, a law professor, aims to construct a rights-based jurisprudence that basically supports the liberal rulings of the Warren and early Burger Courts and would take constitutional law even further in the direction that Ronald Dworkin would push it. *Lochner*'s specter still hangs over substantive due process, however. And as a liberal legal theorist, Richards must find an alternative constitutional basis for his political theory. He thus reclaims the Fourteenth Amendment's privileges-or-immunities clause, which the Court essentially wrote out of the Constitution in the *Slaughter-House Cases* in 1873.

Richards grounds the protection of fundamental rights on the privileges-or-immunities clause by arguing that the antebellum radical abolitionists' advocacy of a Lockean respect for inalienable human rights imbues the Reconstruction Amendments. His reading of the history behind the privileges-or-immunities clause ostensibly demonstrates that the radical abolitionists' "once implausible interpretative attitude toward the Constitution of 1787 [became] the only plausible attitude toward the Constitution" (p. 146). The Fourteenth Amendment not only embodies a commitment to the protection of fundamental rights and the "dissenting conscience," according to Richards but also holds out "an interpretative challenge to each generation's creative powers of universal moral reason and imagination" (p. 147).

Of course, Richards is not the first to take the privileges-or-immunities clause seriously. Laurence Tribe, Sanford Levinson, and Clarence Thomas, among others, have sought to revive that guarantee. But Richards may be the first to advocate such a bold extension of that clause, along with the Fourteenth Amendment's equal protection clause. To be sure, Justice Hugo L. Black argued that the privileges-or-immunities clause was better suited than the due process clause for serving as the basis for applying the Bill of Rights to the states. Richards agrees. Still, he goes beyond that to argue that the clause is so open-ended in the protection it affords that it "must be interpreted with sensitivity to each generation's most reasonable understanding of its circumstances" (p. 231).

On Richards's analysis of the Fourteenth Amendment, the equal protection clause would both permit affirmative-action programs for African Americans and require them for women and homosexuals because they have historically suffered "unjust degradation" (p. 198). Poverty would become a "suspect classification," contrary to the Court's rulings. Human rights to "minimal subsistence, shelter, health care, [and] education" (p. 245) would also find a constitutional home in the privileges-or-immunities clause.

Richards preaches to his choir and will fail to persuade the unconverted. Little attention is paid to readings of the legislative history of the Reconstruction Amendments that run contrary to his argument. Indeed, competing interpretations and evidence advanced by the likes of Raoul Berger, Charles Fairman, and Earl Maltz are largely ignored or dismissed out-of-hand as "implausible" (p. 215).

Moreover, Richards claims to be innovative by adopting an interdisciplinary approach that weaves together law, history, and political philosophy. Historians, he says, "pay relatively little attention to the deeper questions of moral, political, and constitutional theory," while contemporary political and legal theorists are taken to task for being "fundamentally antagonistic to the central normative arguments implicit in history" (pp. 8–9). Alas, Richards's interdisciplinary approach is neither new nor persuasively executed. Richards's book is basically little more than a brief, a law-office history cobbled together in support of his political theory. Too much is erected and depends on too little. Richards's poorly crafted edifice falls under its own weight.

University of Virginia DAVID M. O'BRIEN

Labor Visions and State Power: The Origins of Business Unionism in the United States. By Victoria C. Hattam. Princeton University Press, 1993. 226p. $35.00

Victoria Hattam has written a fascinating book of great importance to all who are interested in the broader questions about the nature and peculiarity of American politics. Carefully researched and closely argued, the book sets out to explain why U.S. labor, in contrast to its European counterparts, adopted narrow business unions rather than broad class organizations. The answer to this question is central for Hattam to understanding what is distinctive about American politics since "The particular path of working class formation in the United States has set the broad contours of American politics" (p. 3).

Building on the insights of other students of American political development and political history, Hattam identifies the unusual power and dominance of the judiciary in the United States. This situation is in contrast to that in England and Western Europe. Hattam presents an especially rich analysis of the evolution of conspiracy law and judicial interpretations of it in the 19th century in this country. She shows that unions in the late 19th century were quite successful in getting some state legislatures, and even at times the federal government, to exempt them and guarantee their rights. State and federal courts, on the other hand, repeatedly undermined and "reinterpreted" these laws, often going against legislative intent, to the great detriment of unions. It was this experience, underscoring for labor unions the fruitlessness of electoral and lobbying activity—even when successful—that turned the American Federation of Labor, according to Hattam, away from politics and towards business unionism.

Hattam's argument, however, is not an account of autonomous state structures. Rather, it was the transformation of the labor movement in the post-Civil War period, and its interaction with the judiciary that led it to change its orientation. Economic development undermined the autonomy of crafts leading to a more proletarian workforce. Labor organizations began to organize on an employee basis, rejecting the earlier producerism which included workers, tradesmen, small businessmen, and farmers as part of the same grouping. Hattam's criticisms of labor historians Sean Wilentz and David Montgomery, who do not recognize the social basis within the 19th century producer-labor movement for the many middle class social reformers is on the mark. Her research on the conflicts within the labor movement between the producer/reformer types and the trade unionist/proletarian forces is informative.

While *Labor Visions* provides important insights on how labor unions became transformed in the late 19th century, its attempt to provide a complete account raises a number of questions. The book overplays the split between the trade unionist organizations and the producer organizations in a way that is unconvincing. Hattam regards the AFL and its predecessors as more Marxist, more class oriented. She sees the AFL rejecting non-workers, while the producer organizations are uniting with small businessmen and rejecting important elements of their class. The producers "generally did not welcome women, blacks, day laborers, and the very poor into their ranks . . . " (p. 18). While this characterization may have some saliency in the period immediately after the Civil War, it does not fit the critical late 19th century period that Hattam wants to highlight. In fact, the late 19th century craft unions were becoming virulently racist (towards Blacks and Chinese especially) and exclusionary, and the Knights of Labor (Hattam's prototype producer organization) was successfully organizing female workers and racially integrating organizations even in the Deep South. Some of this activity of the Knights is documented graphically in works cited in the book (including Peter Rachleff's *Black Labor in Richmond* [1984] and Leon Fink's *Workingmen's Democracy* [1983]).

This misleading characterization of the Knights of Labor points to an even deeper problem with this important work. The rejection of alternative theories is much too glib. Hattam rejects arguments that identify questions of race and ethnicity and the structure of American politics (e.g., two party system, winner take all, lack of proportional representation). She argues that "Neither theory provides a convincing account of the nature and timing of the shift in AFL strategy at the turn of the century" (p. 26). Such an argument, especially with respect to race and ethnicity, is inaccurate. The end of the century was precisely such a turning point for race in American politics, a turning point that had important consequences in law, social organization, politics, and ideology. For labor and other social movements it was critical. The 1890s marked the defeat and destruction of both the integrated Populist movement in the South and the important beachheads of integrated labor organization in that region of the country.

According to E. E. Schattschneider, Northern Republicans, themselves fearful of both agrarian radicalism and northern labor militancy, made a Faustian bargain with Southern Democrats, establishing the System of '96, which held sway stably and unchallenged in American politics for over thirty years. As Schattschneider argues:

> . . . The 1896 party cleavage resulted from the tremendous reaction of conservatives of both parties to the Populist movement, a radical agrarian agitation that alarmed people of substance all over the country. . . .
>
> The new alignment became possible when the southern conservative Democrats decided that they were willing to abandon their ambitions to win power nationally in return for undisputed control of the South. The Solid South was one of the foundation stones of the Republican system because it weakened the Democratic party disastrously and virtually destroyed for a generation the possibility of an effective national opposition party.
>
> . . . The result was that organized party alternatives disappeared in large areas of the country in the North as well as the South. Both sections became more conservative because one-party politics tends strongly to vest political power in the hands of people who already have economic power (Schattschneider 1960, 78–80, italics in original).

Thus, the system of white supremacy in the South, and in the nation as a whole was for several decades one of the foundation stones of national politics.

The period also saw the rise of increased jingoism and national chauvinism around the time of the Spanish-American War. The intensification of racism is reflected in all aspects of American society, not the least important of which was the 1896 decision of the Supreme Court in *Plessy* v. *Ferguson*, upholding the doctrine of "separate but equal." It was a more united, hostile ruling class, along with an intensification of racism, to which the AFL capitulated. Its hallmark became opposition to immigrants, Chinese, Blacks, Latinos, women, and industrial workers, whatever was deemed necessary to protect the privileges of white, male, skilled workers. The alternatives at the time to the AFL were not merely producerist groups, but the later Knights, the Industrial Workers of the World, and within the AFL itself, the United Mineworkers of America, whose interracial, industrial organization was to eventually take it and its allies out of the AFL entirely.

The differences between England and the United States—which Hattam discusses in detail—were not just in the state structures and judiciary, although these differences were, to be sure, there. The differences also had to do with the greater breadth of the English labor movement, a movement in the United States that was undermined by the racist, anti-immigrant, male chauvinist, anti-industrial orientation of many of its constituents and of virtually all of its components and national leadership.

In short, *Labor Visions* is a rich book for all who are interested in the big questions of American politics. Its information and argument will undoubtedly form a key component of any full answers.

Wayne State University MICHAEL GOLDFIELD

Latinos and the U.S. Political System: Two-tiered Pluralism. By Rodney E. Hero. Philadelphia: Temple University Press, 1992. 233p. $49.95.

Hero presents us with a serious and timely book on Latino politics and the shortcomings of both previous scholarly work on the subject and the discipline's attitude toward studies of the politics of communities of color (also see Avalos 1989). This is a well-written and well-researched work. Hero's effort, which, he informs us, is one to enable an understanding of "Latino politics within the larger United States political context and United States politics from the vantage point of the Latino political condition" (p. xi), advances us in that direction. In this pursuit, the larger purpose of the work is both to demonstrate weaknesses in previous approaches and to suggest a new approach that takes these weaknesses into consideration.

Hero argues that "Latinos are in a disadvantaged position politically, socially and economically in the United States" (p. 11). This statement is an early indication of an institutionalized disjuncture in society that he defines as "two-tiered pluralism." In introducing us to this "new perspective" seeking to advance an understanding of Latino politics, Hero indicates that "Latino politics, as well as that of other minority groups, is best understood through a perspective of two-tiered pluralism. . . . Because [of] historical, socioeconomic, and other factors, minority individuals and groups have largely been relegated to a lower social and political tier or arena" (p. 29). This theory, therefore, is being primed to explain how and why Latinos and other minorities have been relegated to a lesser place in the United States political system and, by implication, in economic and social life.

Much of the strength of this book resides in the author's critical summaries of "several major theories"—pluralism, coalition bias–elite theory, and internal colonialism—as applied to the study of the "Chicano political experience." He finds all of them, in varying degrees, wanting, although elements of each are absorbed into his own approach. He dismisses much of pluralist theory, most specifically the argument that multiple access points exist for all interest groups seeking to be influential, while suggesting that coalitional bias (a modification of elite theory) has value in its stress on the importance of social stratification beyond simplistic elites and masses formulas to better understand group power relationships. Hero also assesses internal colonialism as a perspective that has assisted in the understanding of minority politics, although viewed by many as a paradigm (and relic) of the 1970s, through the theory's focus on racism, cultural genocide, and external administration, which are largely or entirely omitted in other theories.

The author develops an argument for the "unique" quality of Latino politics—and the necessity for scholars to understand this quality—into a predominant theme of the book. Part of his dissatisfaction with pluralism, especially the early variety, is that it does not contend with group uniqueness, which suggests to him the "inadequacy of conventional pluralist interpretations of Latino politics and the need for alternative frameworks" (p. 56). Intermittently throughout his book, Hero contends that a historical, as well as a social, background is necessary in order to place and understand the "Latino political situation" in perspective. Noting that there are "important differences" among Latino groups—Mexican Americans, Puerto Ricans, and Cubans—the author is also interested in pointing out similarities. Nevertheless, he also suggests that "formative (historical) events" have occurred in the political development of each group that have explanatory value in understanding each group's political situation and politics. Hero discusses a variety of issues regarding this Latino uniqueness theme, including perceptions of being discriminated against, group self-identity and self-labeling, partisan orientations, participation variables, policy concerns, interest groups, ideology, ethnicity, and class.

Although consistent with his stated structural purpose of the book, Hero's sections on Latino politics in national and state governance (chap. 5–6) are, nevertheless, largely "forgettable." He presents us with structurally awkward chapters that represent somewhat of a contrast and intrusion to his impressive early and closing chapters. Perhaps a stronger commitment to exploring and assessing Latino grassroots and community politics, political organizing, and coalition efforts might have been structurally more appropriate for an understanding of Latino politics. His conclusion, drawn at both national and state levels—that Latinos lack political clout—continues to be rather obvious to, and widely shared by, those studying Latino politics.

The assessment of the elections of Federico Pena in the 1980s (chap. 7) is of interest and should have applicability to Latino and African-American candidates for major local elective positions, although it might have been structurally better subsumed under Latinos and urban politics (chap. 8). In fact, outside of its being a primary personal field of investigative research for the author, it is not entirely clear why this chapter stands structurally alone. Nevertheless, within the context of Latinos and urban politics, Pena's 1983 election as mayor was, as the author claims, significant for at least three reasons: (1) Pena's election marked the first time a Latino was elected to a powerful position in a city whose Latino population was relatively small (about 18% of the total population and 10% of the voting population); (2) Pena became the first Latino elected as mayor in a "major U.S. city"; (3) Pena's victory was fashioned by an electoral coalition that bears similarities to the coalition formed around Tom Bradley in Los Angeles and Henry Cisneros in San Antonio. Hero's conclusions, drawn from Pena's two election victories regarding candidates of color, are that candidates must (1) get almost unanimous support from minority voters, (2) make "extraordinary" efforts to appeal to nonminority voters, and (3) form coalitions (p. 129). This supports findings of Browning, Marshall, and Tabb's *Protest Is Not Enough* (1984), among others.

Hero's chapter 10, "The Study of Latino Politics: Questions and Issues," claims to offer the first substantial critique of the much acclaimed and broadly used Browning, Marshall, and Tabb theory of political incorporation. While probably true, Hero's critique also serves as a bridge to link with his theory of two-tiered pluralism in the final chapter. The Browning, Marshall, and Tabb contribution to the study of minority politics, has been, according to Hero, adopted and heralded by the discipline in its full mainstream. As an example of "mainstream political science research into minority politics," it exemplifies a particular behavioral approach not previously subject to "extensive critical scrutiny." While acknowledging that *Protest Is Not Enough* "is sympathetic to the minority pursuit of social and political equality." Hero argues that there are problems with the model's approach that typify "problems of commission" (p. 177). Hero's two primary concerns are, How appropriate is the approach in the study of minority politics generally and Latino politics specifically? and How adequate is the approach in such study? On the first point, Hero is concerned that the Browning, Marshall, and Tabb model does not incorporate historical or political perspectives of Latinos into their analyses. According to Hero, being wedded to a pluralist approach, which he labels as "ahistorical," takes the understanding of historical uniqueness qualities away from the field of study and investigation. Additionally, Hero contends that Browning, Marshall, and Tabb largely dismiss the views of other scholars who have studied Latino politics, thereby leaving a void in terms of potentially enriching merging and diverging views with which to react. In terms of adequacy of the approach, Hero argues that Browning, Marshall, and Tabb focus on but one type of power relationship (decisional), whereas others, such as Stone, have argued that there are at least four. Hero also questions the adequacy of the almost exclusively allocational (albeit in "highly redistributive form") assessment of policy responsiveness. In other words, do increases in minority local public sector employment, commission appointments, and contracts, as well as the presence of a police review board ensure the political incorporation of a community and, implicitly, equality? Are these measures of sufficient consequence to ensure incorporation? Or is there a danger that in spreading the spoils of local influence through such channels to minorities, Erie's concern of a "proletarianization of minorities" becomes reality? What do Browning, Marshall, and Tabb mean by "political equality"? Is "political equality" a beginning, an end, or both? Does it necessarily follow that such equality would be achieved through incorporation, as implied by Browning, Marshall, and Tabb? These are significant questions posed by Hero, who argues that the allocational nature of these measures of policy responsiveness do not have much "consequence" for equality of redistribution (p. 183). Finally, Hero hints at the cooptation nature of the Browning, Marshall, and Tabb model of incorporation, using Eisinger's concern that even where policymaking bodies become more racially and ethnically diverse, preservation of the political and economic institutional status quo remains both the goal and the norm.

Hero's concluding chapter lays out his alternative approach of "two-tiered pluralism." Thus, it is an important finale, although not very well-written (it is frequently redundant and somewhat confusing). However, perhaps much of this is intentional, as the author seems to be walking us through a "working out" of his theory. Hero defines two-tiered pluralism as "a situation in which there is formal legal equality on the one hand, and simultaneously, actual practice that undercuts equality for most members of minority groups, even if some individuals register significant achievements" (pp. 189–90). In agreement with points raised by Eisinger, Jackson, and Regalado, the theory assumes that there is only "a marginal inclusion of minorities in . . . political process[es]" (p. 190). The subordinate status of minorities in political life is emphasized where pluralism "exists in form but not fully in fact for some groups" (ibid.). Hero concludes that because two-tiered pluralism reflects, for Latinos and African-Americans, an institutionalized subordinate status in political and economic life, the theory represents an important theoretical and political dilemma for American liberal democracy.

California State University, Los Angeles JAMES A. REGALADO

Civic Environmentalism: Alternatives to Regulation in States and Communities. By DeWitt John. Washington: Congressional Quarterly, 1994. 347p. $39.95 cloth, $21.95 paper.

Environmental Protection at the State Level: Politics and Progress in Controlling Pollution. By Evan J. Ringquist. Armonk: Sharpe, 1993. 243p. $55.00 cloth, $25.00 paper.

Until recently, most studies of environmental policy have focused on federal legislation, its implementation by the Environmental Protection Agency and other federal agencies, and the general deficiencies of "command-and-control" regulation. We now have a spate of new studies demonstrating that exclusive preoccupation with federal policy misses many of the most innovative and important developments in the field of environmental politics and administration. Indeed, both of the works under review argue that policy leadership on the environment, as in many other areas, has been shifting from Washington to the states since the mid-1970s. They provide data showing that this trend accelerated during the 1980s as a result of Reagan's "new federalism" and severe budget cutbacks, which ironically led to increased funding and adoption of new, more stringent environmental programs in many of the states. Both books suggest that "we are entering a new era of environmental policy" (*Civic Environmentalism*, p. 1) in which "many states have been far more active in controlling pollution than has the federal government" (*Environmental Protection*, p. 4).

What is most valuable about these works is the empirical evidence they are able to muster for this perspective, using entirely different methodologies. DeWitt

John's *Civic Environmentalism* rests on three lengthy, detailed case studies in which states have become the principal actors: reduction of agricultural chemical use to protect groundwater in Iowa, protection and restoration of the Florida Everglades, and negotiation of new energy conservation policies by electric utilities in Colorado. By contrast, Evan Ringquist's *Environmental Protection at the State Level* employs rigorous statistical analysis to explain variations in state policy outputs and outcomes. These works thus complement each other nicely and together add richly to our understanding of state policymaking and of emerging patterns of intergovernmental relations that include much bottom-up innovation.

John organizes his book around the concept of "civic environmentalism," which he claims as a label for a cluster of five traits that he argues constitute a "new paradigm" of environmental policymaking: (1) bottom-up, rather than top-down; (2) directed toward a new generation of policy problems (ecosystem protection, pollution prevention, cross-media and nonpoint pollution, and sustainable economic development); (2) using a new set of policy tools (mostly nonregulatory and noncoercive, relying on collaboration, negotiation, education, information-sharing, technical services, public investment, and economic incentives); (3) stressing interagency and multilevel cooperation and coalition building; (4) avoiding confrontation; and (5) seeking to create new federal–state–local partnerships in addressing specific problems.

John elaborates and evaluates the strengths and limitations of this model at some length but is clearly advocating it as a desirable alternative to conventional federal regulation (though he is careful to state that it can only complement, not fully replace, the latter). He also has two useful chapters discussing the background of federal policies and budgets and presenting an overview of state environmental programs. But the heart of the book is the three case studies mentioned. These are detailed descriptive accounts (perhaps more detailed than need be on some points) illustrating many of the processes he attributes to his conceptual paradigm. The main disappointment is that his preoccupation with processes and actors does not leave room for as much evaluation of the success of the policy outcomes as one would like (clearly none of the three problems had been resolved at the time of writing).

Ringquist, on the other hand, sets out to explain both state policy efforts and their relative success. In doing so, he follows in the long tradition pioneered by Thomas Dye and others that attempts to explain variations in cross-state policy performance by regressing various output indicators against standard economic, demographic, and political institutional variables. But this is not just another such correlational study applied to state environmental policy. Ringquist takes a quantum leap forward by critically evaluating and refining traditional methodologies for tapping and integrating different kinds of variables that explain variations in state policy efforts and then by comparing these output levels with their actual outcomes in reducing air and water pollution. In other words, he asks not only why some states are much more active in addressing environmental problems than others but also whether state environmental policies matter. Do they work?

This adds up to an "integrated theory of state policy" that stands as an important contribution to the literature in its own right (set out in chapter 4). Ringquist also provides a very succinct and useful summary of federal pollution control policies and state responsibilities under the existing regulatory framework. He then devotes separate chapters to explaining variations in state air pollution programs, air-quality outcomes, water pollution programs, and water-quality outcomes.

In constructing his output models, Ringquist draws on the best of comparative research on economic determinants (Dye, Sharkansky, Hofferbert, Lowry), political variables (Plotnick, Winters, Erickson, Wright, McIver), and group theory and regulatory policy (Lowi, Wilson, Meier, Wood). He critiques each of the traditional models and then integrates them into a comprehensive approach that is operationalized as both a single regression equation and, most significantly, as a causal model using path analysis to compare the weight of each of the most significant variables on policy outputs. These models are then run using standard measures of the strength of state air and water pollution regulatory programs as the dependent variables.

The results are quite interesting. Regressions show socioeconomic variables, political variables, and group influence variables all to be significant predictors of state air pollution programs. None of the traditional models is initially superior to the others. But when integrated in a causal model, the single best predictor of the strength of air-quality programs turns out to be state economic wealth, followed by legislative professionalism and the strength of polluting industries (all of which correlate positively). While the results tend to reject the "agency capture" thesis, they also indicate that factors such as opinion liberalism and the strength of environmental groups matter little. When it comes to water pollution regulation, on the other hand, per capita income and legislative professionalism turn out to be far less significant predictors than two interest group variables: mining industry strength (which correlates *negatively*) and agricultural industry strength (which correlates *positively*). State opinion liberalism is slightly more important when it comes to water policy than for air, but environmental group strength remains a weak determinant.

Finally, Ringquist attempts to determine whether relative policy strength (the dependent variable in the output model) translates into effective pollution control outcomes. The dependent variables now are (1) coefficients for both emissions and ambient concentrations for air pollutants and (2) pollution concentration levels only for water, in each case measured across the time period 1973–75 to 1986–88. Such data are notoriously suspect, but Ringquist probably does as well as possible in constructing his outcome measures.

The results are even more interesting than those for policy outputs. For air pollution, both regression and causal models show that state regulatory policy strength is a powerful determinant of emission reductions and air-quality improvement. Industrial variables have an offsetting impact. For example, fossil fuel consumption is the single most important determinant of nitrous oxide emissions, as one might expect. But overall, state air-quality efforts seem to pay off. The results for water pollution are far less reassuring. There is no significant correlation between policy strength and improvements in water quality. In fact, for some pollutants, regulatory strength correlates *negatively*. But Ringquist argues that strong state policies are a response to poor water quality, rather than vice versa. This confirms that traditional point-source regulation is inadequate for controlling water pollution. Indeed, Ringquist concludes that neither state nor federal regulatory policies will succeed without further strengthening and without more fundamental changes in society.

Both of the works under review add greatly to our understanding of the role states now play in environmental policy. Both are well written and would make excellent texts in upper-level undergraduate and graduate courses in environmental politics, public policy, or state government. My own feeling is that Ringquist has probably taken aggregate analysis about as far as is worth going and that more detailed case studies like John's will add more to our understanding in the future.

Carleton College NORMAN J. VIG

Civil Rights and the Idea of Freedom. By Richard H. King. New York: Oxford University Press, 1992. 269p. $35.00.

This book treats the concern with freedom that animated the Civil Rights movement. By way of advancing an Arendtian interpretation of the Civil Rights movement, Richard King announces early on that civil rights struggle led movement activists to forge genuinely interesting conceptions of what freedom is. An Arendtian approach stresses that politics, in its richest and most meaningful form, is about public justification and explanation of one's political actions, sometimes in dangerous circumstances, before other people—one's comrades and opponents and others who might join in one side or another. The Civil Rights movement becomes, in King's view, a central episode in the development of democratic thought.

King, a reader in American studies at the University of Nottingham, has personal experience of the movement and studied oral histories located at Howard University. His knowledge of African-American literature and political thought back to the antebellum period is also rather good.

King's most basic claim is that everywhere in the Civil Rights movement people were talking, singing, writing poetry, and thinking about freedom (not equality). Buildings or offices that were movement headquarters were called "freedom houses," not "equality houses." Equality tended to present itself, instead, to white observers and sympathizers as the movement's primary concern. This contrast between what was really on the minds of movement activists and what many whites thought the movement was about is central to King's book.

Among other fresh features of the study are the treatment of Martin Luther King's political theory and the sketch of the Student Non-violent Coordinating Committee's (SNCC's) view of the need to construct alternative and highly participatory political institutions. The author suggests some of the scope and power of Dr. King's political intuitions when, in his discussion of Dr. King's approach to civil disobedience and nonviolence, he speculates that Dr. King understood that a history of resistance, suffering, patience, and redemptive thought had deeply prepared black southerners for what he asked of them. The point is not original with the author: one finds it, as well—and far more fully developed—in the thought of Rev. Vincent Harding, the African-American theologian and historian who did so much to develop the now-famous Eyes on the Prize video histories of civil rights and Northern urban struggle. But the point bears repetition, here in this study and elsewhere.

As for the SNCC, the author offers valuable discussions of a key innovation of the SNCC, the "freedom schools," and of the theory about how to build new political identities and personalities that went into the SNCC's political program. The *freedom schools* conceptualized by the great SNCC leader Ella Baker and the related program of *citizenship schools* devised at the Highlander Folk School have been overlooked by movement theorists or democratic theorists with an empirical and institutional bent. As far as I know, the best place to begin studying these interesting institutions in this book.

The discussion of these innovations sets the stage, as well, for an account of how it became possible for some of the SNCC's leaders to adopt a related program—black power—and to become increasingly interested in the connections between violent militance and freedom, in contrast to Dr. King's overarching emphasis on patient nonviolence. The author continues with a close analysis of the origins and character of Frantz Fanon's theory of political violence and with a suggestive treatment and critique of his influence on such SNCC leaders as (then) Stokely Carmichael and on Black Panther leaders, such as Huey Newton. The book ends with rather thoughtful speculation about how the full Arendtian possibilities in the SNCC for establishing durable institutions of continuously liberating popular participation were critically undermined by television's political temptations. The role Arendt assigned to public action and speech could not be realized, King seems to suggest, when several SNCC and Panther leaders chose sound-bite slogans and hot political images of protest over the hard work of ongoing engaged debate and institution building. Although the author does not draw this inference, he may have identified one reason for why, in contrast, Dr. King's speeches, styles, and concepts are still read, memorized, and used: King, his colleagues, and his followers did the kind of difficult Arendtian politics that would assure them lasting influence.

In all, this is an interesting and, in places, fascinating book. Still, it is worth noting certain problems. The topic deserves better organization and plainer writing than the author gives it. Also, a key work is overlooked, Hanes Walton's 1972 study of Dr. King's political thought. The author also fails to appreciate W. E. B. Du Bois's classic, powerful analysis in black reconstruction of black self-emancipation during and after the Civil War and of the intricate links between this epic black struggle and the possibilities for transforming the social relations that undergirded American politics.

A more fundamental flaw in this study is that promising features of the argument are left undeveloped in favor of tying the interpretation to an overly broad array of loosely related intellectual historical topics. For example, the freedom schools and citizenship schools are crucial to the author's claim that conceptual and institutional innovation at the level of political philosophy occurred during and within the movement. But the author quickly moves past a sketch of them to take up such obscure subjects as Alexandre Kojeve's theory of the master-slave relationship and Lacanian psychiatry's concept of the "mirror stage" and their relationship to the evolution of Frantz Fanon's thought. These are doubtless important matters; and by themselves, the discussions are illuminating. But they are distant from the task at hand.

Finally, though there is a cursory treatment of the Exodus story, the book does not explore whether there is a relationship between African-American religious thought and tradition, on the one hand and, on the other, the possibility and evolution of the innovations he investigates. Taking up the issue might have permitted treatment not only of mainstream religious traditions but also of Garveyism (the largest mass movement among African-Americans in this century) and its relationship, if any, to the democratic heyday of the Civil Rights movement.

Still, it is a measure of this book's contribution that it genuinely stimulates reflection about its important claim that the democratic revolution caused by the Civil Rights movements was about more than the innovative protest tactics that have rightly attracted analysts of collective action and social movements. The movement was also profoundly innovative at the level of political thought.

Swarthmore College RICHARD M. VALELLY

The Speaker and the Budget: Leadership in the Post-Reform House of Representatives. By Daniel J. Palazzolo. Pittsburgh: University of Pittsburgh Press, 1992. 262p. $39.95.

How has budget process reform changed the role of the Speaker? How important are individual characteristics in understanding a Speaker's leadership style? These two questions form the heart of Daniel Palazzolo's *Speaker and the Budget,* the most extensive analysis to date of the role of the Speaker in the new budget process.

The budget reform act of 1974 left vague the Speaker's role and many other aspects of the new budget process. Consequently, Speakers have had to adapt. Palazzolo argues that Speakers since budget reform have taken on a shifting set of roles. These roles include the nurturer, who seeks to calm fears about the change in established congressional procedure; the process manager, who attempts to keep budget making timely and prompt; and the opposition party leader, who articulates, protects, and advances the policy and political interests of the majority party. A Speaker is likely to be performing more than one of these tasks, and the tasks will sometimes clash. The role assumed at any particular time depends on policy, political, and institutional conditions and the individual characteristics of the Speaker. Among the more important of the latter are his level of budgetary policy expertise, conception of leadership, and policy preferences. Palazzolo departs from literature arguing that Speakers are either creatures of contextual influences or individual masters of congressional destiny. Instead, he shows that while conditions will largely determine what role is most appropriate at a given point, individual characteristics will influence the manner in which that role is conducted. The author acknowledges that the Speaker's ability to exercise policy leadership is usually constrained (not least by the demands of the membership), but he also suggests how Speakers can overcome this constraint.

The bulk of the book applies this theoretical framework in great detail to the postreform period through 1988. Carl Albert performed a largely nurturing role, educating members of Congress about the new process, reassuring committee chairs that the new system was not designed to challenge their authority but rather to produce an alternative to the president's budget. As Tip O'Neill took over the Speaker's responsibilities, policy controversies, especially within the Democratic party, were escalating. O'Neill became a process manager; and as with Albert, his individual characteristics and open, participatory view of leadership fit the new demands well. Once Ronald Reagan assumed the presidency, O'Neill increasingly emphasized his role as opposition party leader. In both roles, Palazzolo shows that O'Neill enjoyed limited success. Finally, Jim Wright as an opposition party leader inclined toward an activist leadership style because of conditions, his own characteristics, and the brief tolerance of House members.

Palazzolo includes individual characteristics in his framework, but he is careful not to be overwhelmed by them. His emphasis on context is particularly useful as a counterweight to other studies that perhaps exaggerate the influence of postreform leadership. Similarly, Palazzolo draws a nuanced picture of the relationship between the membership and leadership. Palazzolo shows nicely how leaders have managed to be both well ahead of, and well behind, members over remarkably short periods. Finally, the book provides substantial detail, is well written, and uses interviews intelligently. This is one book that does not ask the reader to endure innumerable excepts from the author's interview notes.

A few problems weaken the force of the argument. First, the author does not deal directly enough with the classic problems of research of this kind: What do you do when the *n* is 1? Where are the counterfactuals? Are conditions ever truly "similar" for different Speakers at different times? Although they are to some degree at the heart of the entire book, these problems deserve a more focused consideration, along with any implications they may pose for the author's findings. Second, the book ignores that by the mid-1980s, the budget had weakened as a macroeconomic management tool. The agenda was to reduce the deficit and allow monetary policy to drive the economy. Did this momentous transformation—certainly a new condition—have an impact on the budget process? A third problem is the awkward treatment of activist policy leadership. To Palazzolo, this leadership is apparently neither a role nor an individual characteristic but an undefined combination of both. Unless one wants to assume that only certain individuals can respond to crises with dramatic and activist policy leadership, policy activism is best treated as an additional role. Finally, while the book does not overstate the power of the Speaker's individual characteristics, the author too willingly accepts that Ronald Reagan's personal persuasiveness, charisma, and congressional liaison continually turned the tables in the budget fights of the early 1980s.

Shortcomings aside, the argument in *The Speaker and the Budget* is sophisticated and often subtle. The book would be of interest to budget scholars and students of Congress generally.

University of Wisconsin, Madison JOHN J. COLEMAN

Congress Resurgent: Foreign and Defense Policy on Capitol Hill. Edited by Randall B. Ripley and James M. Lindsay. Ann Arbor: University of Michigan Press, 1993. 337p. $54.50 cloth, $18.95 paper.

In recent years, a growing list of books and articles has become available focusing upon the role of Congress in the foreign policy process. This is appropriate, in light of the fact that the decade or so following the Vietnam War was an era of forceful legislative activity in foreign affairs.

Sponsored by the Mershon Center on International Security and Foreign Policy at Ohio State University, this study emerged from a 1990 conference of some 30 scholars interested in the legislative role in the fields of foreign and national defense policy. The result is a symposium of 12 chapters dealing with various dimensions of the subject. In addition, a comprehensive bibliography provides a highly useful aid for those desiring to engage in further research on the topic.

As is nearly always the case with symposia, this one has certain rather predictable strengths and weaknesses. Its compass is quite broad. Nearly every significant aspect of the subject is covered, sometimes in more than one chapter. The work reflects a wide diversity of viewpoints, with no evident ideological bias. For the most part, the authors exhibit detailed knowledge of their particular topics, and their presentations are usually well organized and readable. On balance, this work makes a useful starting point for those seeking deeper insight into the policymaking process. It should also serve as a valuable guide for officials of foreign governments, who find the American concept of separation of powers strange and difficult to comprehend.

As indicated, this study also has its limitations and will, in certain respects, leave the informed student of American foreign relations dissatisfied. In the first place, in many chapters the documentation is sparse, to the point sometimes of being nonexistent. At times, the reader wonders precisely what sources the author or authors relied upon for their data and conclusions.

Second, as with nearly every symposium, there is repetition in the treatment of the subject; too often, valuable space is devoted to covering familiar ground. Expressed differently, the editors of this volume might well have devoted greater attention to clarifying the level of readership for which the study was designed.

To illustrate the problem, in too many instances, the reader gains the definite impression that dynamic legislative involvement in dealing with diplomatic and national security issues is a new phenomenon that *began* with the post–Vietnam War period—which is assuredly not the case. To the contrary, the tradition is as old as the American republic itself. From the evidence provided by these essays, however, one would surmise that the authors are unfamiliar with General George Washington's difficulties in dealing with the Continental Congress during the Revolutionary War, unaware of the momentous impact of the "War Hawks of 1812" upon the nation's external policy, and oblivious to the crucial role played by Congress in shaping American foreign and defense policies throughout most of the period from World War I to World War II. In brief, one gains little or no sense in these pages that legislative influence has been vigorously asserted a number of times in American diplomatic history, frequently to be followed by periods of executive predominance in dealing with foreign policy problems.

A third weakness of these essays collectively is failure to recognize a salient point made by Joseph White at the end of his treatment of the role of the subcommittees on defense and foreign operations of the two appropriations committees on Capitol Hill. White draws a distinction which always needs to be kept clearly in mind when evaluating the activities of Congress with respect to foreign affairs. He informs us quite correctly that "increased influence" by the House and Senate in external affairs is "not the same as making policy." There may be (and unquestionably has been) evident growth in the former, without necessarily an increase in the latter.

It can hardly be doubted that since the Vietnam War, especially, the *influence* of Congress upon the nation's foreign and defense policy has expanded. As several of the authors emphasize, it is difficult to imagine a return to a time when this was not the case. In recent years, it has become axiomatic to assert that the line traditionally separating "domestic" from "foreign" policy has all but disappeared. Nearly every activity undertaken by the House and Senate has some remote bearing upon the nation's foreign relations.

This is an essentially different matter, however, from saying or implying that Congress is capable of *managing or conducting American foreign policy* or that it has now achieved a level of true (and presumably permanent) "partnership" with the president in the decision-making process. In most of these essays, this distinction is blurred, leading to the misimpression that the advocates of an assertive Congress have actually achieved their goal of gaining a position of equality with the executive branch in dealing with external policy issues.

Yet two recent developments call this conclusion into serious question (and neither of these receives significant attention in this volume). One is the disuse into which the War Powers Resolution has fallen since its passage in 1973. Today, many legislators themselves acknowledge that the resolution has failed to achieve its purpose and that in some respects, it may well have *strengthened the position of the president* in the foreign policy process.

The other development is the record of congressional acquiescence in President Bush's forceful leadership during the Persian Gulf War. Anyone who examines the course of that conflict is compelled to conclude that as indicated earlier, the impulse toward legislative assertiveness in the foreign policy realm *is a cyclical phenomenon*. It would be difficult to find a more conspicuous example of a dominant presidential role in foreign affairs in responding to an external challenge. And one of the most striking facts about the case is that during and after the Persian Gulf conflict, little organized opposition was discernible in the House and Senate to this reality. Implicitly, in other words, even a majority of legislators has acknowledged that the legislative branch is incapable of serving as the focal point of decision making in responding to an external crisis. In the main, events since the Persian Gulf War have supplied additional evidence supporting the distinction between congressional influence and legislative management in the foreign policy field.

Louisiana State University CECIL V. CRABB, JR.

Mobilization, Participation, and Democracy in America. By Steven J. Rosenstone and John Mark Hansen. New York: Macmillan, 1993. 333p. $15.00.

This book considers familiar questions: Who participates? Why has participation declined? It uses familiar data. Yet, in its rather unassuming way, it offers a dramatic reframing of the way American politics researchers think about involvement in politics. At least one strand of research on social movements has traditionally relied heavily on the notion that mobilization (intentional activities by elites designed to gain support or generate resources from target groups) is the central "cause" of individual or group activity. This research has seen the demographic and attitudinal attributes of individuals primarily as contextual factors that may inhibit or encourage the kinds of social relations that lead to social and political action. As one example among many studies in this tradition, McCourt's *Grass Roots Politics* (1977) described how women in working-class Chicago neighborhoods were drawn into neighborhood organizations by group leaders and how aspects of these women's lives (e.g., whether or not they had children or their husbands were supportive mediated the mobilization relationships.

In contrast, behavioral political science research has seen participation as being a set of essentially individual-level decisions shaped largely by such individual attributes as education, income, civic values, and political efficacy. Much of this difference in perspective stems from the different research methods used by sociologists interested in social movements and political scientists interested in participation: while the former have often relied on participant observation and interviews with group members and leaders, the latter have investigated participation primarily by means of survey research.

Steven Rosenstone and Mark Hansen's new book uses the traditional methods and data of behavioral political science (National Election Studies [NES] and Roper surveys) to support a new and distinctive theory of participation that *does* allow "the strategic choices of politicians, political parties, interest groups and activists" to play a central role. Their argument is that "people participate in politics not so much because of who they are but because of the political choices and incentives they are offered" (p. 5).

They argue for the "centrality of strategic mobilization," though they extend and build upon a tradition that focuses upon "resources, interests, identifications, and beliefs of individual citizens" (p. 5). This two-pronged argument—accepting much of the logic and evidence of traditional "attribute-based" research but trying to build in a focus on mobilization—is laid out in more detail in the second chapter. Using NES data, the authors examine group differences and trends in various types of participation since 1952. Using Roper surveys conducted between 1973 and 1990, they trace the trends in contacting (writing to a senator or representative), attending local meetings, and signing petitions. They find seasonal cycles in governmental participation but conclude that neither the short-term fluctuations in participation nor the long-terms trends can be adequately explained by changes in demography or attitudes.

Rosenstone and Hansen inventively employ a variety of data to illustrate the importance of issues and elites in generating mass-level political activity. For example, they use NES data on whether or not the respondent was contacted by a party and also Congressional Quarterly data on the competitiveness of particular elections. For example, the presence of a contested gubernatorial campaign (in presidential election years) means that citizens are 5% more likely to vote. Those citizens in the midst of a competitive House race are more likely to contribute money to parties or candidates, as well as to vote. Another example is the authors' use of McAdam's 1982 tabulation (in *Political Process and the Development of Black Insurgency, 1930–1970*) of civil rights activities, over time and by region, to examine the extent to which these social movement activities exert a mobilizing (or countermobilizing) effect. In these analyses and in their analysis of the correlates of the *contact by parties* variable in the NES surveys, they find that political mobilizers target their efforts strategically—in particular, concentrating on people who have resources, strong party affiliations, or who are "centrally placed in social networks" (p. 210). Thus, they conclude that people in associations are more likely to *be mobilized*, rather than just *to participate*—a subtle but important distinction that brings elites into the picture.

Toward the end of the book, Rosenstone and Hansen attempt to solve the "puzzle" of declining voter participation. They create a "simulation," in effect, which attaches to the respondents in the NES surveys of the sixties the attributes of present-day voters (more education, less efficacy, less partisanship, more skepticism about candidates, less integration into communities, etc.). (I should note that even fairly complicated analytical methods are explained coherently and concisely.) The resulting decomposition of the decline in turnout indicates that two factors (change in voter registration laws and increased education) should have increased turnout by 4.6% and that less than half of the decline can be traced to Americans' declining efficacy, weakened social involvement, increasing youth, and decline in partisanship. The major part of the decline they attribute to declining electoral mobilization: parties "abandoned the labor-intensive canvassing methods of the 60s" (p. 218), electoral competition declined, and demands on campaign resources intensified (i.e., the number of contested presidential primaries doubled).

Though I disagree with some of the assumptions here (e.g., the analysis in chapter 3 essentially assumes that the relationship between race and participation has remained constant since 1952) and feel that the book was unduly repetitive (e.g., the argument about why education increases participation is repeated far too many times), I believe that Rosenstone and Hansen's argument and analysis will—or should—introduce a new and fruitful way of conceptualizing and investigating political participation.

Syracuse University Kristi Andersen

Does Redistricting Make a Difference? Partisan Representation and Electoral Behavior. By Mark E. Rush. Baltimore: Johns Hopkins University Press, 1993. 172p. $32.50.

Does redistricting make a difference? Not according to Mark E. Rush in this insightful look into partisan gerrymandering and its impact on fair representation. Indeed, his central thesis is that a partisan gerrymander is "in fact virtually impossible to prove; furthermore, its impact is unsubstantiated by political science research" (p. ix). Noting that a group's opportunity to be represented is dependent on "durable, identifiable groups of voters," the author points to the paucity of evidence that the parties-in-the-electorate are, in fact, durable or identifiable. Arguing that the variability of partisan voting behavior has been overlooked by the judiciary, as well as political scientists, he contends that a gerrymander cannot have an impact regarding representation of a group if voters of that group are likely to change their partisan behavior in subsequent elections.

Rush's work is well organized and follows a logical progression. Beginning with theoretical considerations revolving around defining a gerrymander, the courts' rather confused rulings, and the contributions made by political scientists, he then shifts to an empirical analysis intended to demonstrate that partisan voting is not constant but rather in a state of flux. Rush does an excellent job of portraying the Supreme Court as a body that has been inconsistent and contradictory. Noting that the Court has been unable to delineate what is meant by a political group and fair representational opportunity, he condemns the case law as "laden with unclear terminology, confused concepts, and conflicting precedents, as well as incorrect assumptions about voting behavior, political partisanship, and representation" (p. 11). Tracing Supreme Court decisions from *Colegrove* v. *Green* through *Davis* v. *Bandemer*, Rush offers a clear and concise account of the Court's difficulties in moving from an explicit individual right to vote to an implicit group (esp., partisan group) right to vote. His prescription: the courts should stay out of this political thicket.

As a political scientist interested in gerrymandering, I was enlightened, provoked, and forced to reexamine my own assumptions by Rush's review of "political science model" (chap. 3). Drawing from what might be considered the classic works on American electoral behavior, this chapter attacks our conceptions of party identification and party voting. Focusing on the debate among the authors of *The American Voter*, Key, Burnham, and Nie, Verba, and Petrocik, voters are portrayed as sometimes partisan, sometimes not; maybe rational, maybe not; driven by issues, driven by candidate appeal; and so forth. It is precisely this failure of political science research to provide a conclusive, fixed profile of all voters that undergirds Rush's central thesis. Quite simply, a number of factors may influence voters and changing the electoral environment may cause changes in partisan behavior. Indeed, based on the research findings, even constituencies that show no change in their partisan voting patterns following redistricting cannot be assumed to be an identifiable, cohesive voting bloc. They may have been affected by "(1) the strong partisan ties on the part of voters, (2) the similarities of the candidates and issues in the new and old districts, or (3) a low level of sophistication and interest on the part of the affected voters" (p. 54).

While I agree that a number of factors influence an individual's voting behavior, there are several problems with this approach. First, noting that some (perhaps many) voters switch does not preclude the existence of identifiable partisan voting blocs within a district who do not. These groups could well be the target of a gerrymander. Second, Rush ignores the possibility that one strategy of the majority party in drawing new maps might well be to move new voters into a previously stable minority-party district to create an element of unpredictability, thus typing up party and candidate resources. Finally, this perspective presents political scientists with somewhat of a no-win situation. Stated in the extreme, if the party split stays the same following an election, it is because of the similarity of candidates and issues; if the party split changes, it shows there are no durable partisan voting patterns. I am left wondering how durable, consistent partisanship could be demonstrated. Unfortunately, Rush does not tell us. He should examine the possibility that a partisan gerrymander may be a matter of degree, not dependent on stable voting patterns either statewide or even districtwide.

These difficulties are not intended to refute Rush's theoetical considerations. Rather, they point out that he does not carry those considerations far enough. His basic premise that durable partisan voting patterns are central to an analysis of gerrymandering is still intact and provides a nice lead-in to the empirical section of this book.

To examine the consistency of partisan voting behavior under similar and differing circumstances, Rush utilizes election results for Congress, governor, and the state senate in Connecticut and Massachusetts from 1972 through 1986. His initial analysis concerns the suitability of statewide measures of partisanship. Through the use of regression analysis (with towns as his unit), he demonstrates that party registration figures for both states are remarkably consistent over the entire period. The previous year's party registration levels predict the current year's with an R-squared over .90 in every case. However, election data indicate a much more volatile electorate. The Democratic percentage of the vote in the previous election does not serve as a particularly good or consistent predictor of the Democratic vote in the current year for the offices studied. For example, Connecticut's congressional election outcomes, "indicate that marked changes occurred not only in the statewide propensity to vote Democratic (shown by the broad fluctuations in the betas) but also in the uniformity of the state's yearly partisan swings, as shown by the wide range of the value of the R-squares" (p. 82). His general conclusion here is that states can be quite heterogeneous and that partisan behavior is not necessarily consistent over time or between levels of office. Consequently, statewide measures of partisanship are not suitable for demonstrating a gerrymander.

Recognizing the inadequacy of explaining the current year's vote with the previous year's, Rush next incorporates variables tapping the impact of incumbency, contested elections, and redistricting. His findings go to the heart of his critique of the assumptions made by studies of partisan gerrymandering. Assuming that a partisan voting bloc can be divided to prevent a majority from forming or that a particular legislator can be defeated by merely shifting voters ultimately depends on the likelihood that those voters will "maintain their partisan profile" (p. 103). Rush's analysis refutes this likelihood: "In fact, all redistricted towns display one common characteristic: regardless of their previous partisan behavior, they all experienced increases in partisan vote levels for the party or incumbent controlling their new district" (p. 103). Not only did the partisan vote increase, but the increase was often greater than that which occurred in towns remaining in districts held by that party's incumbents.

These data reveal an important finding. When the political environment changes, voting behavior changes, whether it is due to incumbent retirement, redistricting, issues, or weak partisan attachments. To the extent that this fluctuation in voting behavior is meaningful, Rush's thesis that a gerrymander cannot occur is upheld. To demonstrate a gerrymander that is discriminatory necessitates identifying blocs of partisan voters who are moved strategically to the detriment of the party out of power. If those voting blocs are defined on the basis of election results but their members do not vote out of partisan considerations (or if they do, not consistently), then the underlying assumptions needed to demonstrate a partisan gerrymander are unfounded, as are claims of unfair representation by the "out party." This is precisely what Rush maintains and his data show. However, there are several shortcomings with his analysis that need to be examined.

A focal point initially addressed by Rush is that a statewide pattern of partisan voting (i.e., a base vote) is not a good indicator of the behavior of subdivisions within a state. In part, this contention is supported by the results of gubernatorial elections. Yet the race for governor is an election involving high visibility, issues that are more likely to catch the voters' interest, and a higher propensity for candidates to deemphasize the party (since they are faced with an electorate composed of a large proportion of "independents" who have access to other-than-party cues). Can we reasonably expect election results to reveal a clear, consistent, partisan voting pattern? I would prefer an analysis examining election data for a less visible statewide office where a greater reliance on the party as a voting cue could be assumed. It is this type of office that is generally used by both individuals drawing district maps and students of the impact of redistricting. If the findings of such an analysis were similar to those reported by Rush, I would have more confidence in their validity.

A second drawback with this book's empirical section has to do with the unit of analysis. Rush uses election data for towns in both Connecticut and Massachusetts throughout the period under study. A better approach would rely on much smaller units, such as precincts. There is no effort to distinguish between small or large towns, nor is there any indication of the degree to which some towns may dominate a legislative district while others are relatively insignificant to the district's electoral makeup. It may well be that some areas in a town *are* stable bastions of party support, with others exhibiting frequent voter fluctuation. Utilizing towns as the unit of analysis not only masks this pattern but also presupposes that when drawing district maps, the cartographer creates districts through a reliance on aggregate votes from towns—an unlikely supposition. This difficulty primarily affects the author's use of bar graphs to show variable patterns of town voting over time. However, a similar observation applies to his regression analysis wherein the party vote in legislative districts is to be explained. If the vote in some areas of a district exhibits a historically partisan stability but is highly volatile in other areas, this pattern will remain hidden if the vote is aggregated by district.

A final criticism of Rush's empirical analysis has to do with his apparent view that partisan gerrymandering is an all-or-nothing endeavor. There is no attempt to differentiate districts that might be most amenable to a shift in voting blocs from districts that would not. Nor does he take into account the possibility that a "successful" gerrymander might require only the strategic shift of a relatively small number of voters in a few districts. For example, even in his analysis of the Connecticut state senate elections of 1982 (a redistricting year), he provides no a priori reasoning as to what we should expect in the towns which were involved in new districts. Were the new districts dramatic alterations of the old? Did the voting patterns in the new districts reveal a consistent, across-the-board change in partisan voter behavior throughout the town? These and related questions cannot be adequately addressed from the approach employed in this study. Does this invalidate the work by Rush? By no means.

Despite the aforementioned criticisms, *Does Redistricting Make a Difference?* is worth reading. It provides evidence that the assumptions made by legal experts and political scientists regarding the possibility of, and the consequences from, partisan gerrymandering may be faulty. It raises serious theoretical questions about concepts such as "fair representation" and "politically relevant groups." Finally, a careful review of this work yields fresh insight as to how to demonstrate the existence of a gerrymander. Can a partisan gerrymander make a difference? I think so, but I agree with Rush that we have failed to illustrate how.

Ball State University GARY L. CRAWLEY

Prejudice, Politics, and the American Dilemma. Edited by Paul M. Sniderman, Philip E. Tetlock, and Edward G. Carmines. Stanford: Stanford University Press, 1993. 339p. $45.00.

Race is the keyword that connects the twelve chapters brought together in this volume. Not much connected otherwise, most have something to offer taken individually. A strength of the volume is its cross-disciplinary character, particularly important for this area of research.

The chapters by psychologists attempt the greatest generalization (as is typical when human nature is the primary disciplinary focus). Rothbart and John provide an excellent summary of three important findings from experimental research on stereotypes: (1) even random labeling of individuals as members of groups (the "minimal group paradigm") tends to bias memories and behavior; (2) stereotyping, though a normal cognitive process needed to simplify the world, often leads to confounding of description with evaluation; and (3) positive contact with an outgroup member may not reduce stereotypes, for the effect may be to distinguish the individual from the negatively viewed group, rather than change the image of the group. The authors also present longitudinal data on stereotypes by students in a large university and note that the absence of a positive characterization (e.g., black students are *not* described as "scientific") can be meaningful, given the reluctance of students today to voice negative stereotypes in questionnaire settings.

An even more ambitious attempt to generalize is provided by Sidanius and Pratto under the heading of social dominance theory. Racial and other forms of oppression occur, they hold, because all societies develop group-based hierarchies for evolutionary reasons (e.g., allocation of scarce resources). The argument is most convincing when the authors cite the dominance of one ethnic group by another in many societies throughout history. Their own research evidence seems less persuasive: it involves mainly individual difference variables, yet their theory is primarily at a macro level. In addition, because of the authors' highly general aims, the theory has little to say about variation over time within a country like the United States.

The least relevant psychological chapter is Gough and Bradley's report of research using classifications of individuals by their spouses and acquaintances in terms of words like "cynical" and "intolerant." The aim is to avoid the limited validity that can be ascribed to self-report of ethnic attitudes. Although an interesting idea, the authors relate their results only to a variety of psychological scales developed by the senior author, not to ethnic attitudes or behavior. Thus, one problem of validity is replaced by another.

With the exception of Rothbart and John's data on college students, none of the chapters by psychologists throws light on the present state of racial attitudes in the United States. This is the province primarily of the political science and sociology contributors. Thus, Dowden and Robinson present information showing that 18- to 25-year-old Americans no longer provide a major source of increased liberalization of white racial attitudes, as was found in earlier research. However, the youngest white cohorts remain more liberal than older cohorts on most issues despite the twelve Reagan–Bush years, which suggests a half-full, rather than half-empty, interpretation of the same data.

Three chapters by varying combinations of the volume editors and associates focus directly on political conflicts within the United States. The basic argument is that in addition to earlier issues of equal treatment versus personal prejudice, there are now other issues that invoke political disagreements of a nonracial character, over the implementation of a social welfare agenda and various forms of affirmative action. Opponents of what are often seen as problack positions in these controversies are regarded by the authors as motivated primarily by political beliefs that transcend race, not by covert prejudice. For example, Carmines argues that the same appeal to individualistic values that helped the civil rights struggle three decades ago is now used to oppose affirmative action.

There is merit in the premise of these chapters: as Lipset and others have argued, it is misleading to assume that all issues involving race can be reduced to a conflict between bigotry and egalitarianism. Yet even if Americans do hold abstract ideological beliefs that resist preferential treatment for blacks, one notes that preferential treatment for one's own interests (e.g., continued high social security benefits for wealthy retirees) manage nevertheless to be justified. Individualistic values are more "flexible" than the editors credit, depending on who is the recipient of some special benefit.

A different attempt to distinguish among issues within the broad racial debate is presented by Kluegel and Bobo. They employ factor analysis to discern patterning of attitude items in the National Election Study, the General Social Survey, and ABC/*Washington Post* polls. The chapter demonstrates both the virtues and limitations of such an approach; for though it provides a wealth of information on a massive amount of data, the outcome is clearly contingent on the scattering of items available to the authors. One of the more interesting later results is that while age and education show moderately large associations with traditional measures of prejudice, associations of the same variables with beliefs about the prevalence of discrimination are much smaller.

The remaining four chapters in the book concern African–Americans directly and do so with varied questions in mind and with quite different methods. The most relevant in terms of the title of the book is "Ethnic Stereotyping," a secondary analysis of black stereotypes about other minorities. Sigelman, Shockey, and Sigelman find that in comparison to whites, blacks do not differ significantly in their stereotypes of Hispanics but do hold more negative stereotypes of Jews. There are also racial differences in the demographic correlates of stereotyping. For example, age is positively correlated with stereotyping for whites but not for blacks.

Hochschild contributes a broad review of the distinctive economic, social, and psychological problems faced by middle-class African–Americans in a white-dominated society. The chapter offers a mixture of anecdote, evidence, and speculation, which is useful but sometimes unclear as to what weight should be given to particular conclusions. Stanley's careful analysis of supporters and opponents of Jesse Jackson as a presidential candidate acknowledges that it is impossible to know the attitudinal sources of white opposition to Jackson (e.g., how much reflected racial prejudice). The chapter is of value, but would fit better into a book on voting behavior.

Last (but definitely not least) is Hauser's investigation of why college entry among African–Americans declined sharply from the mid-1970s to the mid-1980s. In addition to learning the most sensible answer to this question, readers will benefit from a knowledgeable critique of some popularly voiced claims (e.g., that as many black men of college age are in prison as are attending college) and will come to appreciate the limitations and pitfalls of government data for this and other important issues.

University of Michigan HOWARD SCHUMAN

How Women Legislate. By Sue Thomas. New York: Oxford University Press, 1994. 205p. $35.00 cloth, $14.95 paper.

Sue Thomas has written a book that all institutions will want for their libraries and many political scientists will want to acquire for their course work and personal collections. Thoroughly researched and deftly presented, *How Women Legislate* provides incontrovertible evidence that women in politics do make a difference. While these differences may be less transformational than some of the earliest feminists envisioned, they are probably much more substantial and impressive than those unfamiliar with the literature of women and politics have assumed. What makes this book uniquely valuable is that it takes "what women want" out of the realm of conjecture and grounds it solidly in the goals which women state legislators express for themselves. Thomas then goes on methodically to demonstrate how these aspirations do (and do not) differ from those of their male colleagues and then further documents the impressive extent to which these aims are being realized.

Thomas begins this study with as succinct a summary as one is likely to encounter of the longstanding and perplexing differences, even among contemporary fem-insists, as to whether women would be better served by an equality strategy or by a strategy that acknowledges and protects women's special capacities and roles. From this necessary grounding, Thomas neatly segues into a central question that has frequently been posed but rarely answered: Will women confront or adapt to the status quo? Will they contest the existing "rules of the game" or go along to get along?

Drawing primarily on her own interviews, questionnaires, and observations but also on a very comprehensive search through the relevant literature, Thomas contrasts the somewhat reticent early women state legislators with their much more self-confident, ambitious, and policy-producing contemporary counterparts: "In the 1970's, women state legislators, for all their acknowledged hard work and preparation, did not engage in active participation in the everyday world of legislatures. . . . In contrast, women of the 1980's, while acknowledging similar obstacles, clearly plowed through them and moved on to contribute in all areas of legislative life" (p. 83). Interestingly, the later women legislators have developed increasingly, rather than decreasingly, distinctive policy priorities from those of their male colleagues.

One of the book's most intriguing and important aspects is the evidence of just how rapidly women lawmakers have strengthened their self-regard and increased their policy successes in a relatively short period of time. Drawing on the findings of both psychology and sociology, Thomas convincingly explains these differences primarily through the power of proportionality. Interlegislative comparisons make it very clear that as women move from token minority status to significant (albeit still minor) presence, there will be reduced pressures for conformity and increased possibilities of and support for distinctive behavior. Thus, as women have become more numerous, they also become increasingly assertive about pressing their longstanding interests in legislation of special consequence for children, women, and families. Obviously women are still a long way from dominating or even being consistently successful in influencing the state public policy agenda, but Thomas supplies sufficient historic background to make this relatively sudden explosion of empowerment (sudden in contrast to the 72-year struggle for federal suffrage) not just notable but astonishing.

Thomas next examines why, given the increased self-confidence and policy effectiveness of today's women legislators, there has been little tangible change in the legislative process, a goal that many women (but relatively few men) legislators articulate. If the institutional reforms (as compared to the policy accomplishments) to date seem somewhat disappointing, Thomas attributes that disappointment more to the unrealistic expectations of those who predicted and expected radical change than to the timidity of the women officials themselves. She also effectively employs some quick case studies of congressional reform to demonstrate the formidable obstacles facing institutional revisionists.

Stylistically, some of the author's points are reiterated more than is necessary in this relatively compact volume. Substantively, the failure to even mention the Term Limits movement, which will clearly have profound impact on state legislative institutions generally and, in particular, on women's chances both for election to, and leadership within, these institutions is disappointing.

However, Thomas has, by answering some of the central but heretofore largely unanswered questions about women in politics, moved the entire discussion beyond where it has been churning to important new ground. Since her book includes the questionnaire and interview instrument used in her research—and a very comprehensive bibliography—those interested in tracking the next stage of developments will find that Thomas has provided the tools and the directions for them to do so.

University of Arkansas DIANE D. BLAIR

COMPARATIVE POLITICS

U.S.-Mexico Relations: Labor Market Interdependence. Edited by Jorge A. Bustamante, Clark W. Reynolds, and Raul A. Hinojosa Ojeda. Stanford: Stanford University Press, 1992. 495p. $52.50.

This edited volume is the fifth in the series issued by the United States–Mexico Relations Project, a consortium of American, Canadian, and Mexican scholars organized by Clark Reynolds at Stanford. In 19 chapters, 25 researchers explore aspects of what most believe is a "binational" labor market linking the United States and Mexico. The linkages between Mexican and U.S. labor markets have been strengthening for most of the twentieth century. During the 1980s, it was possible correctly to call the linkages between particular U.S. industries and Mexican workers a binational labor market, as some fruit and vegetable farms, hotels, and restaurants, and janitorial firms became dependent on Mexican workers in the same manner that Wall Street depends on commuters from New Jersey and Connecticut. This 1992 book includes the papers presented at a conference held in 1986. The contributions thus reflect the concerns of the mid-1980s: authors' references to contemporary events are full of U.S. labor shortages, a (then) proposed immigration reform and control act, and a Mexico that was just taking its first steps to switch from inward-looking to outward-oriented economic policies.

Readers should turn to this book primarily for its perspectives on how U.S.–Mexican labor interdependence was seen in the mid-1980s. Some of the themes remain important: the Mexican drumbeat that illegal immigration is due to more U.S. demand–pull rather than the story of Mexican supply–push, the evolution of many Mexican communities so that they are dependent on the U.S. labor market and remittances earned there, and the undermining by desperate Mexican immigrants of wages and standards in U.S. labor markets. Other contributions reflect ephemeral fashions, including assertions that the United States will, by 1990, surely "need" to import Mexican workers to fill vacant jobs.

Few people will read this five-hundred page book from cover to cover. For this reason, I highlight the points raised by the various contributors to guide readers to chapters of interest. The editors grouped the papers into four sections: a conceptual framework, supply-and-demand for migrant workers, migration's impacts on Mexican and American communities, and sectoral dynamics. In the book's last chapter, Clark Reynolds explores whether—and how fast—wages will converge in what is likely to be a more integrated North American labor market.

The editors are well-known researchers with a point of view. Their introduction emphasizes the importance of Mexican-born workers in the U.S. labor force (Mexican immigrants account for more than 10% of U.S. labor force growth); the key role of remittances in the Mexican economy (exporting people generated as much foreign exchange as exporting oil in the mid-1980s); and the apparent neat match between a youthful Mexico and an aging United States. (According to the editors, the United States "will be facing a serious shortfall in labor supply . . . into the next century" [p. 2].)

In order to better understand labor market linkages between the United States and Mexico, the contributors were asked to explain who Mexican migrant workers in the United States are and what effects they have on the United States and Mexico. Contributors were charged with evaluating the benefits and costs of labor migration for groups such as employers, workers, and consumers. Finally, contributors were asked to assess the likely effects of NAFTA on these labor market linkages. Contributors were also asked to evaluate IRCA, the U.S. effort in 1986 that legalized almost three million aliens and attempted to reduce future illegal immigration.

The first three chapters argue that restrictionist U.S. immigration policies reflect fallacious or elite beliefs about the effects of illegal immigration. Jorge Bustamante, for example, contrasts to "U.S. public opinion," which wants illegal immigration stopped, the "economic reality" that such immigration is beneficial (p. 23). Bustamante outlines four likely scenarios and their migration consequences, from rapid U.S. economic growth and Mexican political stability (which, according to figures cited but not included in the book, would lead to more female and urban migrants) to a U.S. recession and instability in Mexico (which, according to a fairly detailed elaboration of U.S. responses, would lead to a "border wall and moat") (pp. 33–34, 39).

Heller's contribution is a lengthy summary of progressive and corporatist U.S. history, which, in his view, made immigration restrictions a necessary adjunct to industrialization, the creation of a working class, and unions. Garcia y Griego argues that a few officials at the apex of Mexico and the United States have been able to make immigration decisions relatively unfettered by societal pressures (p. 76). In a fascinating series of seven cases, Garcia y Griego shows how the U.S. government twice initiated Bracero programs to bring Mexican workers legally to the United States: once in 1917 and again in 1942 (p. 85). In his discussion of these cases, I think Garcia y Griego gives too much credence to "fears" that without Mexican workers, the United States war effort might have been in jeopardy. Many agricultural subsidies, including foreign workers, were initiated during wartime "emergencies" and then maintained in peacetime, from Bracero programs to mohair subsidies (to make soldiers' coats).

Six papers and one comment make "Supply and Demand" the longest section of the book. Hinojosa Ojeda and McCleery describe a CGE model they constructed to stimulate the effects of various types of North American economic integration. Three types of economic integration are simulated—protectionism, neoliberal opening, and managed interdependence—with the authors clearly favoring managed interdependence, rather than the neoliberal NAFTA. According to their neoliberal model, changes in capital and labor (migration) policies have greater effects on production than do changes in trade policy, reinforcing the argument of those who said that NAFTA was more about investment than trade.

Wayne Cornelius, perhaps the best-known North American migration researcher, summarizes data from a number of Mexican communities during the 1980s to demonstrate that Mexican sojourners or temporary workers are increasingly U.S. settlers. Hayes-Bautista and his colleagues explore California's demographic future, projecting a median age of 39 in 2030, a minority population of 62%, and a Latino population of 40%.

Hayes-Bautista and his colleagues emphasize that working-aged Latinos will be expected to contribute to the health care and social security costs of older white workers, and they may be unwilling or unable to assume this "burden of support" if they do not have the education needed to earn high incomes. According to the authors, 57% of Latino adults in California have less than a high school education, versus 22% of non-Latinos (p. 204). This chapter, too, succumbs to the tendency in this book to sketch out "scenarios" involving young Latino workers versus elderly white voters.

Bortz notes that Mexico has always had high inflation, low wages, low productivity, and a smaller share of its population in the workforce than did the United States. Alba joins most of the other contributors to assert that U.S. demand for Mexican workers is what created a binational labor market between the two countries. Reyes notes that even optimistic assumptions about growth and job creation in Mexico leave a deficit in the year 2000 of 5 to 10 million jobs (p. 266).

Chavez examines the effects of Mexican immigrant workers in San Diego in a chapter that includes several reminders of how dated some of the material is. He quotes a mid-1980s study's assertion that in 1990, migrants from other states and countries would be needed to fill 60% of the jobs created in southern California (p. 275), although the story of the early 1990s is job losses and the emigration of middle-class Americans from the area.

Alarcon contributed an interesting case study demonstrating that once labor migration takes root, it sets in motion processes that make it "self-perpetuating." Gonzalez-Arechiga argues that among Mexicans living in border areas, border commuters (with U.S. immigrant visas) are much better off than those who seek work without visas in the United States—precisely the intent of U.S. immigration policies such as IRCA. The mid-1980s is reflected in Muller's paper, which opens with newspaper accounts of "labor shortages" and goes on to describe how the United States can, with immigration, offset "insufficient growth of native-born workers to meet projected employment needs" (p. 369).

"Sectoral Dynamics" includes four loosely connected chapters, dealing with the shift of U.S. auto parts and assembly operations to Mexico, the substitution of nonunion Hispanic janitors for unionized blacks in Los Angeles, a description of California agriculture's depending on Mexican workers while criticizing Mexico for trying to step up exports on the basis of low wages, and a concluding essay posing the important question, Will free trade lead to a convergence that pulls U.S. wages down toward Mexican levels or Mexican wages up toward U.S. levels? Reynolds promises to answer the question in a forthcoming book.

University of California, Davis PHILIP L. MARTIN

Political Culture and Democracy in Developing Countries. Edited by Larry Diamond. Boulder: Lynne Rienner, 1993. 455p. $49.95.

After a quick start in the sixties, progress in the study of political culture seems to have lagged. Most of the new work on democratization has ignored the construct altogether, focusing on institution building and elite maneuvering instead.

But now that attention has begun to shift from the emergence to the durability of democracies, political culture has come back into play. The dismissal of values as deterministic preconditions now seems misguided. The rejection of political culture as a welter of extraneous preferences that get in the way of template-like theorizing is reminiscent of the scholastic fanaticism of Giles of Viterbo, declaiming in the face of the Reformation that "men must be changed by religion, not religion by men."

The volume on political culture and democracy edited by Larry Diamond therefore comes at an auspicious time. It gathers 13 original articles on the workings of political culture in over 20 countries. The collection contains some excellent pieces. One of the most insightful, by Gehad Auda, analyzes Islamic movements and resource mobilization in Egypt. Auda chronicles the decay of the symbolic capital, the legitimacy of the secular regime, and the growth of the social capital of religious fundamentalism through the takeover of existing institutions and the creation of alternative political networks. Christine Sadowski provides a cogent analysis of the growth of civil society in postcommunist settings. And the introduction and conclusion provided by Larry Diamond are perceptive reviews of the literature.

The rest of the book, however, does not match the standards set by these essays. Too much of the volume rehashes ideas from the sixties with methods from the fifties. Indeed, one of the reasons why the editor's orienting and wrapup essays work as well as they do is that he makes only sporadic reference to what comes in between. This is not unusual in compilations devoted to big-picture topics. A similar deficiency afflicted the series on democratization put together some years ago by O'Donnell, Schmitter, and Whitehead. The fact that many of the case studies function mainly as casual grist or white noise to a larger framework does not necessarily detract from the interpretation, but it does raise doubts about the wisdom of packaging it in book format. One senses that an editorial admonition to keep things jargon-free was translated by some of the contributors as an injunction to dumb it down. The resulting air is that of a textbook or of an uninspired briefing for distracted foreign service officers. All the things that critics find wrong with commentary on political culture—(ad-hocery, discursiveness, casual *apercus*, etc.) are present here.

Yet the flaws of *Political Culture and Democracy* need not be identified as inherent to research on political culture. To some extent, studies in political culture are simply no longer the flavor of the month, having fallen out of fashion with one or another faction in the discipline. Toward the end of the eighties, Samuel Popkin argued against the misuse of culture, only to wind up insisting on the importance of "ethos"—the same thing under a different name. Culture bashing of this sort keeps academics off the street, but it does not add to the body of knowledge.

In addition, some difficulties may be distinctive to a stage of development in the field—at least insofar as it bound up with comparative politics, rather than intrinsic to the approach itself. While survey research, the instrument of choice in the breakthrough days of political culture, has become routine almost everywhere, the goals of cross-national collaboration have probably become more ambitious. It is no trick to pull off one-shot surveys in hitherto inaccessible places, but the career trajectories of many foreign scholars and the vagaries of social science funding agencies do not encourage a commitment to data gathering over the long haul. Without comparable time series, it is difficult to sort out blips in public opinion from transformations in political culture.

Nowadays, the field operates according to a two-track model that can be understood partly as a pair of adaptations to disparate political circumstances and partly as indicative of deliberate choice. Innovative work in comparative political culture (e.g., Merelman on the United States and Canada, Putnam on Italy, and Scott on Southeast Asia) comes out of a semiartisanal, eclectic tradition of research that draws on tools from rational choice theory, cultural anthropology, and even the occasional survey. By contrast, with the exception of some of the work of Inglehart and his collaborators, the results of large-scale team projects are mixed. The payoffs that archival data sets and a big-science style eventually produced in the study of American mass politics have yet to appear on the horizon of comparative politics.

The chief problems, however, are conceptual. For the most part, Americanists who study mass politics traffic not in political culture but in political psychology and in an environment where academics worry not so much about the stability as the quality of democracy. The paramount questions still revolve around individual consciousness, and the dependent variables are usually precise and microlevel. The volume edited by Ferejohn and Kuklinski, *Information and Democratic Processes* (1990), exemplifies this approach: theoretically focused, methodologically sophisticated, with lots of measures polished over long periods of time.

By contrast, analysts of political culture generally favor a more Durkheimian view, and many of them work in erratic contexts where lack of funding is not the main thing preventing replication. Collective norms and aggregate cleavages are the units of inference, and the dependent variables tend to be macrocosmic (e.g., regime stability) even when the empirical materials are drawn from individuals. One may criticize political psychology, for its implacable individualism, as "typically American," as well as for a certain imaginative fatigue. All the same, the sheer nebulousness and noncumulative treatment of political culture is daunting.

In effect, political culture has become a baggy monster. The proliferation of idiosyncratic understandings of the phenomenon evident in the volume at hand may reflect dissatisfaction with the narrowness of earlier formulations. But the seminal work of Almond and Verba at least had a consistent theory and a rigorous methodology. The country studies collected here have neither.

Nevertheless, with a bit of pushing and hauling, some coherence can be made to emerge from a consideration of thirty-odd years of work in the field. A heuristic conceptualization of political culture might profitably come in three parts. First are sets of hegemonic values, the preeminent norms of one society or another (e.g., "individualism" in the United States). Next are dissident or counterestablishment orientations. No majoritarian norms are permanent; and even when such norms are massively "characteristic," no society is so homogenous as to obliterate ideological countercurrents. Societies, in other words, have cleavages, latent or otherwise. This focus has important consequences. Emphasis shifts from the sorting of individuals (participant, subject, parochial, ideologues, near-ideologues, etc.) to the multidimensional classification of issues and cleavages. Third, polities have—or claim to have—what Axelrod has called metanorms—overarching rules for adjudicating conflicts between hegemonic myths and countervalues.

This capsule rendition of political culture is incomplete. To begin with, we need to know what cleavages are about. There seem to be a limited number of axial themes: protocols of authority versus values of autonomy, the priority given to accumulation as compared to standards of equity, and "everything else," including preconceptions about the boundaries of the political and the private sphere.

Another problem is that in practice, this theoretical approach presupposes verbalization that is literacy-based. The demonstration that mass publics do not know what they are talking about much of the time fails to deal with this bias. The further challenge, then, is to encompass predominantly oral societies and subcultures, to come to grips with the elusive pervasiveness of the electronic media in mass societies, and to interpret iconic environments and semiotic landscapes that are expressive in ways that political scientists are not trained to examine. This McLuhanesque condition requires the cultivation of a wider range of methods and sensibilities than most academic training imparts.

Such an agenda regarding political culture may lapse into a story about how difficult it is to tell a story—that is, toward a linguistic turn and a methodological obsessiveness of supreme interest to a few academics and incomprehensible to the vernacular world it tries to interpret. This is the perennial danger of the hermeneutic circle, that of projecting unverifiable fancies onto demotic cues. But once it is granted that the steady monitoring of mass politics that has been a hallmark of social science in industrial societies is not practicable in the same way in more tumultuous settings, alternative and possibly complementary strategies must be taken seriously.

Arizona State University PETER MCDONOUGH

Democracy and Development. By Axel Hadenius. New York: Cambridge University Press, 1992. 218p. $44.95.

Axel Hadenius' *Democracy and Development* is a worthy successor to Robert Dahl's *Polyarchy* (1971) in its effort to quantitatively evaluate the extent and the determinants of democracy across nations. In particular, this book focuses on the "varying *level* of democracy at a certain point in time (1988)" in 132 Third World countries (p. 2). OECD and East European countries are excluded.

Democracy and Development is really three books in one. The first quarter of the book is an extended, thoughtful essay on the varying criteria that philosophers and political scientists have posited for democracy and the difficulties involved in operationalizing those concepts. The second quarter of the book develops a multifaceted "snapshot" index of democracy as practiced in individual countries at a particular point in time. Hadenius finds a U-shaped pattern, most countries having relatively low or high scores, with few in the middle. The second half of the book reviews the arguments and evidence about the effects of a number of possible influences on the level of democracy, including economic development (notably measures of socioeconomic modernization, economic dependence, and inequality), culture, and institutional background (limited to the role of the military and colonial history). Regression analysis is used to test these variables against the author's index. Hadenius argues that trade with the United States, percentage of the population that is Protestant, and capitalism have a strong positive association—and economic development (as measured by literacy) a weaker positive association—with democracy, while commodity concentration, ethnic and linguistic fragmentation, and military expenditures are negatively associated with it. The final chapter of the volume includes an excellent discussion of problems of attributing causation in quantitative cross-national analyses of democratization.

Readers will find each of these sections valuable. I especially recommend the second part of this book, not just for students of comparative political development but for use in courses in political science methodology. *Democracy and Development* is much like David Mayhew's *Divided We Govern* in the care and exceptional clarity with which the reader is guided through the author's thought process, using an informal, almost musing-aloud style as the author attempts to construct a quantitative index to measure a dependent variable that many have talked about but few have been able to measure in a plausible way. As in Mayhew's book, readers can see all the gears turning in this exercise. This section of *Democracy and Development* also provides a wealth of information on individual countries. And while readers may quibble with details of Hadenius' rankings (e.g., awarding Cyprus a perfect 10 democracy score despite the forced division of that island seems a bit of a stretch), they will, justifiably, be widely used by other scholars.

Most of the limitations of the book flow from its analytical method. Reduction of the many facets of democracy (most notably participation and contestation rights) to a single metric conceals as well as reveals. As Hadenius acknowledges, the focus on a "snapshot" view of the existence of democracy at a particular point in time ignores questions of democratic stability. The use of states as units of analysis means that disproportionate weight in the regression analysis is given to island microstates. And the analysis risks understating the importance of variables for which quantitative indicators are either not available or contain a lot of measurement error and variables that have a nonlinear relationship with the presence of democracy.

The author generally sticks to the measurable and to the aggregate data. The analysis would have been strengthened by a more detailed presentation of data on individual hypothesized determinants of democracy, including the use of scatterplots to identify potential nonlinear relationships and outlying cases that might disrupt—or illuminate—the statistical analysis. Why some countries and countries in particular regions might be persistent outliers from the regression line is not explored. Why are democracy scores so much higher in Micronesia than in the Middle East and North Africa, for example? Hadenius tends to discount regional patterns (p. 145), but this may be because their effect is being undercut by other variables in his model.

Students of political institutions might wish to see more attention given to specific institutional arrangements, such as presidentialism versus parliamentarism, electoral rules, and federalism. The author's failure to say anything about the emerging democracies and dictatorships of the former Soviet bloc (including the successor states to the USSR) is another curious omission. While their exclusion from the author's data base is understandable, some speculation about what the author's findings suggest about prospects for democracy in those countries would have been welcome.

Such omissions should not be surprising in a relatively brief book, however. While *Democracy and Development* is unlikely to be seen as the definitive answer to debates about the social and political determinants of democracy, it should reinvigorate many old debates and spark new ones. Hadenius concludes his volume with stimulating suggestions for future research. His ideas, data and analytical techniques will—and should—be widely utilized and revised by others to retest and refine, refute, or broaden his conclusions.

Brookings Institution R. KENT WEAVER

The End of Communist Power. By Leslie Holmes. New York: Oxford University Press, 1993. 358p. $55.00.

This volume provides a comprehensive analysis of political corruption in Leninist systems, with special attention given to the fundamental role corruption played in those systems' delegitimization, disintegration, and ultimate collapse. The study is cross-national, in principle bridging all of the two dozen countries that adopted a Leninist system, though with more detailed attention given to the Soviet Union and China. While sensitive to the cultural and developmental differences among these Leninist states, the author is persuasive in identifying numerous cross-national commonalities in norms of corruption. His analysis reveals a complex relationship between level of development and corruption. For instance, the type and pace of economic growth and level of reliance on coercion are critical to manifestations of, and regime responses to, official corruption.

The volume's title is somewhat misleading in that the author does not assess all of the political, economic, and societal factors that contributed to the collapse of Leninist systems. Rather, he focuses on the role of official corruption and the crisis of system legitimacy in assessing the end of communist power. Even with this focus, Holmes is biting off quite a bit, but he is effective in demonstrating the centrality of corruption and legitimacy crisis to the processes that overwhelmed most communist regimes in the late 1980s and early 1990s.

A major strength of this volume is its theoretical treatment of both legitimacy and corruption. These nebulous and imprecise phenomena are often defined in very subjective ways. As a consequence, much work done in this area has been interpretive and anecdotal, rather than systematic. Holmes grounds his empirical study of the Soviet and Chinese cases in a theoretical framework based primarily on legitimation and the concept of crisis. He builds on the literature while providing a more rigorous and useful definition of corruption—a definition advantaged by encompassing both the "hard" and "soft" dimensions of objective and perceptual elements. His taxonomy of 20 forms of corruption spans a wide array of actions (e.g., political patronage, embezzlement, and deliberate dereliction of duties), with each form explored in a broad overview of Leninist countries.

Holmes is clearly one of the few people who have the requisite knowledge base to provide a comparative assessment of developments in most Leninist systems. He is quite effective in illuminating the roots and logic of the anticorruption campaigns that emerged in the USSR, China, and elsewhere in the 1970s and 1980s. Occasionally, his country surveys are rather anecdotal. But the studies of the USSR (1966–87) and China (1966–86) are systematic and well crafted. Holmes makes good use of published reports of official corruption to explore a wide range of questions. The author's cautious conclusions confirm, and add to, our often limited knowledge, demonstrating for both the USSR and China that (1) there was increased reporting of corruption from the late 1970s into the 1980s, (2) an increased level of officials identified with corrupt activities, (3) top-level leadership changes were correlated with reports of corruption, and (4) there was an eventual move to go after high-level Party officials in the Gorbachev and Deng periods. Overall, while this study does not permit Holmes to rank-order, in importance, the factors leading to official corruption, it does allow for an illumination of the systemic and regime conditions conducive to official corruption in the Leninist setting. Some speculation is unavoidable, but Holmes's discussion of the costs and benefits of corruption yields especially insightful conclusions.

We are only now beginning to see the publication of serious studies dealing with the crises of mature Leninist systems, and this volume must be counted among those which effectively illuminate the contradictions that overwhelmed advanced communist states. Holmes demonstrates that Leninist system corruption and anticorruption campaigns were in part a product of a transitional period, in which regimes were moving away from traditional models and ideological formulations toward capitalist system structures and norms. A profound identity crisis of irreconcilable political and economic cleavages overwhelmed regimes increasingly committed to root-and-branch reform. The regimes' own reporting of official corruption became yet another symptom of a general tendency to emphasize performance and what Holmes identifies as the legal-rationale mode. Yet this proved incompatible with the Marxist–Leninist model. As this study indicates, the complex interplay of positive and negative consequences of regimes publicly fighting various forms of corruption (e.g., patronage, second economy) only undercut system and regime legitimacy. Indeed, the political system was weakened by these developments. Institutional cleavages and a general confusion overwhelmed the various party and state agencies charged with addressing problems of corruption.

One of the most interesting features of this volume is Holmes's seven-stage model of political transition to postcommunism. In the process of presenting his model, Holmes offers some fresh and provocative thoughts on communism and postcommunism and their relationship to our understanding of modernism and postmodernism. He argues that in certain respects, the Leninist system represented an extreme form of modernity, while in other respects, it was premodern or "extramodern." At the most basic level, Leninist systems had many modern values (often developed to an extreme level) but few modern forms. The legitimacy crisis, to which he gives considerable attention, was a natural outcome. Holmes concludes that postcommunist states exhibit many features associated with early modernity (e.g., authoritarian and autocratic political tendencies, more distinct class structure with group cleavages), suggesting that these societies will continue to struggle with political and socioeconomic dilemmas long characteristic of the "modern" West.

This volume was authored in the late Soviet period, but the analysis and conclusions nicely anticipate post-Soviet developments now underway in Russia and Eastern Europe. The theoretical discussion suggests why corruption will be of continuing significance to the politics of postcommunist and transitional societies. Having consulted this volume, readers will be in a better position to assess those complex and dynamic developments as they continue to unfold.

University of Arizona JOHN P. WILLERTON

Power Without Force: The Political Capacity of Nation-States. By Robert W. Jackman. Ann Arbor: University of Michigan Press, 1993. 194p. $39.50 cloth, $14.95 paper.

Robert Jackman enters the lists of contributors to state theory with an elegant treatise, admirably succinct and sophisticated. In a mere 162 text pages, he forcefully engages much of contemporary state theory, and stakes out a distinctive niche for himself. Those familiar with the Jackman corpus of scholarship will expect clarity of thought and a penchant for proposing parsimonious categories for complex processes; they will not be disappointed.

The critical vehicle for conceptual purchase over "state," argues Jackman, is the notion of political capacity. The essence of capacity is the ability of leaders to exercise rule without force: in other words, effectively to hold power, understood as comporting authority. Political capacity in turn depends upon two critical factors: legitimacy and organizational age.

Legitimacy rests upon consent of the governed, and is thus implicitly measured by the absence of physical coercion. The Gramscian interpretation of "hegemony" wins an approving nod; civil society for states endowed with an ample political capacity becomes a willing subject. Legitimacy, for Jackman, is not regime-specific; it "simply requires a degree of acquiescence, an acceptance of the political order as generally reasonable, *given the known or feasible alternatives*" (p. 99, emphasis in the original).

Institutionalization is likewise central to political capacity. One important measure is simply survival over time. Institutions, argues Jackman, acquire value in the public eye through their routinization over time and the familiarity of long usage. New states are thus intrinsically fragile. Conversely, organizational age may be taken as a proxy of capacity. The longer a given institutional framework has persisted, the more likely is its continual survival.

The footprints of Max Weber and Samuel Huntington are clearly visible in this line of analysis. But Jackman is not simply a disciple of any of his intellectual predecessors; his particular synthesis of state theory is distinctive and original. His conceptual contribution is rooted in a wide-ranging inventory of political development debates, and a rich knowledge of the empirical world of nation-states. Unlike a large fraction of contemporary state theory, his reflection is not primarily based on observation of the limited universe of European or advanced industrial polities, but is genuinely global in scope.

By methodological disposition Jackman is drawn toward an effort for measurement. Happily the temptation is only half-heartedly pursued. He devotes a chapter to the measurement of political capacity, but exercises appropriate restraint in actually implementing any such design. Legitimacy is at once fundamental and yet elusive, a quintessentially qualitative notion. Proxy indicators such as amounts of physical force or violence, security expenditures, or size of armed forces are dubious measures when stripped from their context. Institutionalization cannot be adequately inferred from so simple a notion as chronological age. State socialism, after all, progressively disintegrated and the decay of legitimacy of Marxism-Leninism as regime ideology was directly proportional to aging. So also has political capacity of many African states progressively declined over time, opening the way to the African variant of the Huntingtonian "third wave" of democratization experiments, however uneven their success.

Perplexing as well is the Jackman aversion to acknowledging that states can be usefully seen as macro-historical actors, an idea which he dismisses as posing "overwhelming problems of reification" (viii). Indeed so, but states do make war or peace, consume important fractions of the national income, engage in "authoritative allocation of values," and issue standing commands in the form of laws. Although their sovereign will may be institutionally fragmented, and is certainly the object of fierce struggle on the part of social groups and leading individuals, at the end of the day actions occur bearing the decisive signature of the state. Equally puzzling is his stigmatization of "historicism" as a regressive analytical trend, as if specificity of context and trajectory were not important conditioning factors to weigh against comparative regularities.

Among the particular attributes of given nation-states which comparative analysis needs to consider but often overlooks is their cultural configuration. Here Jackman deserves high praise for specifically including ethnicity as a variable. Again one may debate the completeness of this dimension of his analysis; more needs to be said about the relationship between the variable importance of identity politics and overall patterns of state legitimacy. But successful accommodation of communal difference is becoming increasingly critical to sustaining legitimation, and even in this decade to state survival.

These reservations notwithstanding, I heartily recommend this slim volume to all students of political development and state theory. The synthesis of evolving scholarly debates since the 1950s on these themes is a masterpiece of compressed yet comprehensive critique. The brief for political capacity as master concept is both stimulating and provocative. The Jackman volume is a major intellectual achievement.

University of Wisconsin, Madison CRAWFORD YOUNG

Money, Time, and Politics: Investment Tax Subsidies and American Democracy. By Ronald F. King. New Haven: Yale University Press, 1993. 536p. $45.00.

Taxation and Democracy: Swedish, British, and American Approaches to Financing the Modern State. By Sven Steinmo. New Haven: Yale University Press, 1994. 280p. $32.50.

Ronald King begins his book with a quote by Joseph Schumpeter that must warm the heart of every scholar who studies tax policy: "The spirit of a people, its cultural level, its social structure, the deeds its policies may prepare—all this and more is written in its fiscal history, stripped of all phrases" (p. 1). Yet the tax side of fiscal history is rarely studied, in sharp contrast to the voluminous literature on government expenditures. One disincentive is surely the complexity of modern tax systems. Tax attorneys spend a lifetime mastering the intricacies of the U.S. tax code. It is little wonder that most scholars have preferred to study policies that can more easily be summarized and compared.

In *Taxation and Democracy,* Sven Steinmo steps boldly into the gap with a comparison of tax policy in Sweden, Great Britain, and the United States. Steinmo organizes his study around both a "common path" and "divergent patterns." The common path consists of rising progressivity of taxation from the turn of the century to the 1940s, as tariffs were replaced by progressive income taxes. Most recently, tax policy has moved in the opposite direction. In all three countries, tax reforms adopted in the 1980s reduced the progressivity of the tax systems.

The divergent patterns consist of quick-and-dirty sketches of the tax system in each of the three countries. The American tax system is riddled with loopholes. As a consequence, the tax system in the United States is both extraordinarily complex and limited in its revenue yield. The Swedish tax system is characterized by stability, high revenue yields, and low taxes on income from capital. British fiscal history is marked by instability, as each new government undertakes to overhaul the tax system and reverse the policies of the preceding government.

Most of Steinmo's analytic effort goes into explaining the cross-national variation. His explanations rest upon characteristics of each country's political institutions. The reason that the U.S. tax code is filled with loopholes is explained by the power and independence of members of Congress and their electoral incentives to champion tax breaks for interest groups who are important sources of political support. The continuity and rationality of Sweden's tax policy is due to electoral rules that allowed the Social Democratic party to maintain a majority of seats in the upper chamber even though the party rarely received enough votes to obtain a majority of seats in the lower chamber. British instability stems from the combination of a first-past-the-post electoral system that exaggerates the impact of slight changes in the vote and centralization of power in the hands of the majority party in Parliament. All of these explanations are conventional and plausible. Explanations can hardly become conventional without a certain degree of plausibility. In the case of Sweden, Steinmo goes beyond the conventional wisdom in his discussion of how the constitutional reform that eliminated the upper chamber in 1970 had the effect of undermining Social Democratic dominance, thus reducing the incentives of the bourgeois parties to compromise on tax policy or any other issue.

The most interesting pattern, however, is the pattern common to all three countries. Steinmo argues that the wave of regressive tax reforms that swept Europe and the United States in the 1980s was a consequence of changes in the world economy that have made it easier for firms to relocate production to avoid unfavorable tax environments. This assertion, however, is too simplistic. Within the standard economic framework for understanding the effect of profit taxes on investment, there are many different ways governments can respond to increased capital mobility. The fact that all three governments responded by broadening the tax base and reducing tax rates requires explanation. In theory at least, governments could equally well have responded by keeping tax rates high and increasing targeted tax incentives for investment in the domestic economy.

It is perhaps inevitable, in a book that covers the entire tax system in three countries from the 1800s to the present, that the descriptions and explanations are somewhat superficial. In *Money, Time, and Politics,* Ronald King devotes twice as many pages to one aspect of the tax system (the taxation of corporate income) in one country (the United States) from 1945 to 1980. This allows King to investigate in much greater depth the economic and distributional trade-offs that constrain tax policy everywhere. The basic dilemma that King explores is between the popular demand for the rich to pay more and high-profit taxes' negative effects on investment, employment, and growth. King describes how Democratic presidents, as much as Republican presidents, sought to rework the tax code to encourage private investment, even at the expense of reducing the effective progressivity of the tax system. The major difference between the parties was that Democratic presidents favored targeted tax incentives that minimized the decline in progressivity of the tax system, while Republican presidents were more likely to favor broad reductions of the tax rate paid by firms and shareholders.

The two authors describe the U.S. tax system in strikingly different terms. King dismisses the numerous loopholes emphasized by Steinmo as being a side-show. The tax revenue lost from the multitude of tax expenditures that benefit small groups is small, King claims. The tax expenditures that reduce revenues significantly (deduction of mortgage interest payments for owner-occupied housing, tax-free status of interest from state and local bonds and deduction of state and local taxes, deductions for employer-provided fringe benefits such as health care and pensions, and tax incentives for capital formation) are either received by broad groups of voters or, in the case of investment incentives, adopted because broad groups of voters stand to benefit from the encouraged activity. While Steinmo focuses on the role of Congress, King sees the president as the most important actor. Indeed, King's argument is that presidents have centralized decision making regarding tax policy in response to the fragmentation of political power in Congress that Steinmo observes.

If King disputes Steinmo's portrait of the United States tax system as incoherent, Steinmo challenges King's apparent belief that the trade-off between investment incentives and distributional effects is less severe in social democracies. In fact, Steinmo's description of Swedish taxes could not be a better illustration of the willing sacrifice of distributional goals for the promotion of capital formation that King uncovers in the United States. yet the normative implication of the trade-off in tax policy between the effective rate of taxation of income from capital and the level of private sector investment is easily exaggerated. The efficiency and distributional effects of the tax system cannot be evaluated in isolation from the expenditure side of fiscal policy. A government that takes in a large share of national income through efficient, regressive taxes in order to finance redistributive expenditures may do far more to reduce income inequality than a government that attempts to tax the rich.

In conclusion, *Taxation and Democracy* is a fast-paced, well-written book that makes interesting comparisons and, for the most part, conventional arguments. Steinmo's book would be an excellent choice for an undergraduate course in comparative public policy. King's book, with 450 pages of text devoted to the intricacies of United States tax policy from Truman to Carter, is unlikely to fit within the constraints of courses taught at any level. Yet scholars who seek a deeper understanding of tax policy would be well-advised to find the time to read *Money, Time, and Politics*. Because King has studied the debate over taxation so carefully, he is able to convey the economic as well as the political considerations that shape the taxation of income from profits in the United States and elsewhere. In the final analysis, the impact of different democratic institutions on tax policy, though interesting, is of secondary importance compared to the economic and political concerns that King documents.

University of California, Los Angeles MICHAEL WALLERSTEIN

Mothers of a New World: Maternalist Politics and the Origins of Welfare States. Edited by Seth Koven and Sonya Michel. New York: Routledge, 1993. 447p. $49.95 cloth, $16.95 paper.

The history of women's involvement in welfare state formation poses interesting questions for both scholars of state building and scholars of feminism. Looking across Western democracies in the late nineteenth and early twentieth centuries, the authors in this volume reveal the important role that middle-class women reformers played in securing state assistance for women, children, and families. This history complicates traditional arguments regarding the role of industrialization and male labor politics in welfare state formation. Further, at a time when today's women's movements tend to emphasize equity and individual rights, the centrality of maternalism to turn-of-the-century feminism raises issues about strategy and identity in women's politics.

Mothers of a New World comes out of a series of conferences that were held at Harvard's Center for European Studies in 1986 and 1987. Many fine scholars participated at these gatherings; and even those whose work is not represented here (e.g., Barbara Nelson, Karen Offen, and Theda Skocpol) have continued to contribute to scholarly work on the issues discussed in this volume. This collection of essays is a significant contribution to scholarship on gender and politics and to the study of political development in Western democracies.

Studies of state building and political development have traditionally failed to consider gender. By focusing on the role of industrialization, militarism, and class formation, women are treated as secondary players in the history of political and institutional change. But as the essays in this volume amply demonstrate, gender and women's politics were important factors in political development of Western democracies. As the subjects of state interest, women in militarist nations (as Alisa Klaus discusses in respect to France) were encouraged to bear children, and in industrial economies with employment concerns, they were constrained from working beside men in factories. As advocates of social provisioning, women gave voluntary assistance to poor families in private programs, which were often models for public services. They also articulated a self-defined feminine sensibility of social caring to justify state aid to women and families. The maternalist politics of women reformers was sometimes associated with efforts to expand women's political rights, sometimes not. Maternalist politics was also sometimes resisted by male politicians and sometimes coopted to please the church, the army, or male unions. The fit between maternalist policies, women's politics, and state building was a politically contested issue.

In feminist theory, there is currently a debate over the logic and effects of strategies of equality and strategies of difference for women in politics. Historical consideration of the politics of maternalism in the late nineteenth and early twentieth centuries is unlikely to settle this debate, for it reveals that maternalism was used both to justify claims of political equality and to legitimate women's exclusion from formal politics. What it also shows (as many feminist theorists, such as Carole Pateman, now argue) is that the difference/equality dichotomy in politics is often a false construction. Surely, maternalism is an irreducible difference between the sexes. Men cannot carry or bear children. Yet the political construction of this difference in Western democracies said little about the resulting political gains and status of men and women. So while there may be a point in critiquing liberal universalism (as many feminist theorists do), the essays in this volume suggest that there is little merit in drawing essential lessons about the politics of maternalism and difference that were associated with a broad range of political efforts and outcomes.

One weakness of many of the essays in this volume is that they consider women as advocates of social policy, rather than viewing them as the recipients of such programs. As Christoph Sachsse says in his essay on German welfare state formation, "Women have always been and remain overrepresented among the recipients of poor relief and welfare" (p. 136). The relationship between women reformers and women clients is complicated not only by class but also (as Eileen Boris notes in her essay on the United States) by race and ethnicity. Such complexities matter, since eligibility for social welfare and the receipt of social welfare both mark the terms under which women are incorporated into the political system. Some women reformers advocated what Seth Koven calls "civic maternalism" as a way of recognizing the contribution of all women to the polity, and therefore of their right to participate in the political process. But as Susan Pedersen shows in her study of France, social provisioning could also be used to further differentiate the citizenship status of men and women, and to weaken the political claims of working-class women by reminding them of their duties to the family and the nation. Thus, it is insufficient to consider gender and welfare state formation from the perspective of women reformers. More work needs to be done on the role of women clients in policy formation (as Molly Taylor Ladd does in her study of the Sheppard–Towner Act in the United States, and Jean Quartaert, in her discussion of class and welfare policy formation among women in Germany) and as citizens whose political status is marked by their position as welfare recipients.

War, economic change, democratic expansion, race and class relations, and gender relations all contributed to the context in which women reformers used maternalist rhetoric to seek new social provisions for women and families a century ago. Despite the vast differences among the countries studied here (France, Britain, the United States, Germany, Sweden, and Australia), they had in common the importance of women's maternalist politics to their early welfare state formation. The consequences of maternalist campaigns may be as varied as the contexts in which they occurred; but in all of these cases, women's political efforts left a substantial mark on social policies and state institutions. This volume is a welcome correction to the previous neglect with which these campaigns have been treated by social scientists.

University of Texas, Austin GRETCHEN RITTER

Retirement of Revolutionaries in China: Public Policies, Social Norms, Private Interests. By Melanie Manion. Princeton: Princeton University Press, 1993. 196p. $35.00

Melanie Manion's extensively researched, neatly organized, and tightly argued book is a welcome addition to our knowledge of Chinese politics, particularly in light of the paucity of monographs dealing with administrative aspects of China's huge bureaucracy. Marshaling an impressive array of Chinese official documents (some obviously internal publications) as well as extensive interviews with both retired revolutionaries and the younger generation of cadres who benefited from the retirement of senior cadres, she successfully manages to incorporate all relevant information, as well as her interesting analysis, within a thin volume of 196 pages.

This book is a case study of a series of policies specifically packaged to induce the voluntary retirement of old revolutionaries. Particularly targeted were cadres who joined the revolutionary movement before 1949. Such individuals (presumably having made a contribution to the foundation of the People's Republic of China), thereafter gradually moved into key positions in the ruling structure, and thereby defined the contour of Chinese politics over the subsequent 40 years. Throughout their careers, these cadres frequently engaged in deadly factional, ideological, and policy struggles among themselves. Many of them fell victim to purges during the Cultural Revolution and were later rehabilitated, reinstated to their previous positions, and then retired through the Deng-era policies that Manion details in her book. Deng Xiaoping's success in removing these aging cadres paved the way for the rise of a younger generation of technocrats, while avoiding the Maoist mode of political purges. Furthermore, this generational change in the Chinese leadership parallels the well-known shift in the regime's goals from Mao's revolutionary changes to the more contemporary pursuit of economic development.

It would be impossible for the Chinese to develop any rational system of managing their huge bureaucracy without first setting up an administrative norm of retirement. For this reason, Manion's detailed analysis of an evolving cadre retirement system—and particularly her conceptualization of this evolution as a process of institutionalizing a regularized administrative practice—is a fresh and welcome approach. It will certainly stimulate scholarly interest not only in retirement but in other aspects of the cadre management system as well. The civil service system that China will hopefully develop in the coming years may be one such area of interest.

As the book's subtitle indicates, Manion's central organizing concept is captured by her term *norm,* which she defines as "a pattern of action, accompanied by an understanding of the pattern as a social standard of propriety," whose violation will invite social disapproval (p. 23). The key question raised in the book asks, "Has a norm of cadre retirement emerged?" (p. 153). If cadre retirement has become a norm, then, presumably, the social expectation and sanction, rather than any enforcement backed by state authority, will work to invoke voluntary compliance of cadres reaching the retirement age.

Manion's answer to the central question is equivocal: "It is fair to conclude that the process [of establishing an official retirement policy] is fueling the emergence of a norm of retirement for the overwhelming majority of Chinese cadres" (p. 153). She attributes this somewhat ambiguous finding to the "ambiguous, ambivalent, inconsistent, and discriminatory" nature of the measures adopted by the Chinese policymakers in their retirement policy (p. 155), a nature that violates some of the theoretical principles from the chapter specifying how norms emerge. Chinese policymakers made too many concessions to the interests of the targeted group at every phase of elaborating and implementing the policy. Nonetheless, she argues, other factors (e.g., the self-interest of both aging revolutionaries and the younger generation of cadres and the wisdom of the strategy of "small wins") operated to help the gradual emergence of effective retirement norms.

Manion's qualified and somewhat conflicting conclusion leads one to wonder whether the special retirement program is an appropriate case to study the norm-building process, particularly along the line of theoretical insights drawn from experimental social psychology. The special retirement program involves too many peculiarities to establish a general system of norms. As the Chinese term *lixiu* ("leaving the job to recuperate") indicates, the policy package included in the program was considered as a one-shot deal only applicable to the old revolutionaries. Moreover, the targets of the policy were members of the most politically influential groups, whose support was critical to the regime. Furthermore, the evolution of the special retirement program can only make sense in the specific Chinese political context of Cultural Revolution purges, rehabilitations, a shift of regime goals, and the renewed effort to streamline the bureaucracy for the new task.

In summary, Manion's approach is innovative, as is her conclusion that special retirement succeeded in changing the social expection of lifelong-tenure for cadres. But at the same time, her framework of norm building also constrains her research and prohibits her from further exploring some of her own interesting findings, such as the relationship she discovers between the retirees and their former work units, which has important implications for understanding the issue of formal authority and informal networks in Chinese politics.

University of California, Berkeley HONG YUNG LEE

Politics East and West: A Comparison of Japanese and British Political Culture. By Curtis H. Martin and Bruce Stronach. Armonk: Sharpe, 1992. 335p. $49.95 cloth, $16.95 paper.

If the title of Curtis H. Martin's and Bruce Stronach's book pays homage to Rudyard Kipling's oft-quoted line, "East is East and West is West, and never the twain shall meet," the theme of this book could not be further from the quotation's spirit. They are not saying that East and West are distinctively different. Instead, they emphasize the similarities of British and Japanese political culture. They argue that consensus is "ingrained as a social and political value" in both Britain and Japan and that it is "an expressive factor of voting behavior" and policymaking (p. 229). Martin and Stronach particularly emphasize the "collectivist traditions and acquiescent behavior" of the British and the Japanese, though they argue this behavior is more pronounced in the Japanese case (p. xv).

They do, however, draw a strong contrast between the historical origins of British and Japanese democracy. They emphasize the longevity and "homegrown" quality of British democracy, in contrast to the recent nature of Japanese democracy. The elite origins of Meiji Constitution and the Allied/American-imposed postwar constitution, they claim, make Japan a less legitimate and a potentially unstable democracy (p. xi). They interpret the differences they find in the political culture of the two countries through the lens of these diverging constitutional origins. Unfortunately, they never adequately draw out the implications of this contrast.

With more than 308 pages of densely printed text (excluding the index and references), 45 tables, and a massive bibliography of some 450 entries, Martin and Stronach must have a sense of accomplishment on seeing their book come to print. A short, well-written introduction describes the key theoretical concept of the book, *political culture.* Their definition of political culture (drawing heavily on Sidney Verba's definition) connotes a system of values, beliefs, and symbols about the patterns of political institutions and interactions in which there are clear linkages between individual behavior and political events (p. x). The language and structure of analysis is clearly framed in the structural–functionalist approach of Almond, Verba, Pye, and others.

The book is divided into three parts: "Political Values and Attitudes," "Political Participation," and "Policy Making." In each, the authors' method is essentially the same, namely, to review extant survey results from both countries. Their comparisons are sometimes made on the basis of a survey administered in both countries. More often, their conclusions are drawn from an assortment of country-specific surveys. (Most of these surveys, it should be noted, were taken before 1989.)

Part 1 focuses on political values selected as representative of the Japanese and British policy, including attitudes toward self, others, and the political system. Their analysis of the relevant survey results provides an interesting snapshot of the cultural similarities and differences in the two countries. These results, however, do not really help us to understand the dynamic construction of British and Japanese political identities. The authors conclude, for instance, that both the British and the Japanese are patriotic but that this national pride is directed toward different targets. Whereas the British are especially proud of their political institutions, the Japanese express pride toward their culture and economic successes. They attribute this difference to the differing political trajectories of the two countries: the defeat of Japan in World War II profoundly affected Japanese trust of political institutions (p. 65). Their argument, although plausible and ostensibly historical, seems to overlook the powerful role that political institutions have had in the construction of a Japanese cultural identity.

Part 2 focuses on Japanese and British attitudes toward political participation. It continues the discussion of the attitudinal basis of popular support and documents the range of participatory activities in Britain and Japan. One of their conclusions is that traditional values continue to play an important role in voting and in the level of political participation in both Britain and Japan. They argue that the political passivity inherent in traditional values has inhibited participatory democracy in both countries (p. 173). Nevertheless, the value given to consensus, they conclude, "has a much greater effect on voting in Japan because it is a so much more deeply ingrained social and political value" (p. 229).

Part 3 continues to look at the influence of values and attitudes through the lens of policymaking. Using their political culture approach, they compare five different groupings of institutional relationships encompassing various configurations of policy actors (ministers, bureaucrats, party officials, legislatures, interest groups, and local government officials). Here, they conclude that "both Japanese and British decision making rest on the foundations of strong elite consensus and control, buttressed by tenacious though different collectivist norms and traditions, and by an acquiescent citizenry" (p. 301).

Unfortunately, there is no concluding chapter. This is where they might have tied together a theoretical argument about the relevance of the political culture perspective. While they acknowledge that the debate around political culture has been controversial, their response is to write that they "shall attempt to keep these criticisms and refinements in mind" (p. xii). Essentially, they sidestep the question of causality (culture causes behavior) but still view political culture as creating limits, providing resources for problem solving, and conditioning behavior (p. xi).

If they had attacked this political culture debate head on, the limitations of their argument would have become clearer. The problem is that the political culture approach (at least, the one they utilize) leads to a skewed view of politics. Their approach inherently overemphasizes consensus and tends to ignore societal conflict, that is, why, when, and how dissatisfaction erupts. Since this is a book published prior to the 1993 rise of Japan's Hosokawa government, it cannot be faulted for making the usual claims for the impervious power of the Liberal Democratic party. However, we can ask how their political culture approach might have helped us to explain this transformation. Essentially, political culture acts as a residual explanatory category—explaining everything yet nothing. Moreover, there is a tendency in this type of approach to ignore or downplay the variations within a society. The result is that conflict between societal subgroups is, for the most part, left unexamined.

Martin and Stronach's book provides a well-written synthesis of survey results. It will be useful for scholars who want to familiarize themselves with recent survey work related to political culture, participation, or policymaking in Japan and Great Britain. Their book will be less helpful in unraveling the complexities of political change in these two countries. In essence, Martin and Stronach have not addressed the fundamental theoretical problems surrounding the structural-functionalist approach to political culture.

University of Chicago SUZANNE B. RYAN

Lessons of Struggle: South African Internal Opposition, 1960–1990. By Anthony W. Marx. New York: Oxford University Press, 1993. 347p. $16.95 paper.

Anthony Marx's study of the South African internal opposition to white supremacy from 1960 to 1990 is illuminating, detailed, and theoretically sophisticated. *Lessons of Struggle* offers an analytical framework that privileges the role of ideology in the making of history. More specifically, Marx argues that the way in which oppressed people interpret their own experiences is decisive in determining their patterns of resistance to repressive rule and their aspirations for an alternative and more equitable social order. Subjective interpretations, however, do not emerge from a vacuum; they are molded and constrained by material constraints. In the tradition of Antonio Gramsci, Anthony Marx emphasizes the mutually conditioning interactions between ideologies and structures in the process of social change. It is from these interactions that oppressed people draw the lessons of their struggle.

In South Africa, under apartheid, race became the defining category: it was the single most important factor in determining a citizen's life chances. Through its total control of state power, the white minority institutionalized racism and monopolized wealth, status, and privilege. The institutionalization of racism confined the black majority to a powerless and destitute existence. Black South Africans were politically emasculated and transformed into exploitable units of labor. Not surprisingly, the indignities of racism and the realities of material deprivations decisively shaped the ideological configurations of black opposition to apartheid.

While race has inevitably colored oppressed peoples' perceptions of the dividing line separating them from their overlords and produced a black nationalism pitted against a white nationalism, Marx cautions us against oversimplifying the ideological orientations of South Africans. *Lessons of Struggle* shows that despite its overwhelming significance, race is not—and has never been—the sole identity to which South Africans have given their loyalty. Ethnic, class, and religious identities have also claimed their allegiances. These competing identities, as Marx insightfully demonstrates, are not immutable: they are in fact quite malleable. Elites manipulate them for their own political ends, but they cannot do so as they please. "Images of collective identity" mobilize people only when they "resonate with the masses under particular historical circumstances" (p. 236). Evoking the power of normative symbols and the experiences of daily reality, such images have contributed to the development of rival ideological families.

For instance, Marx explains how during the early 1970s, the search for "psychological liberation" induced the emerging Black Consciousness movement (BCM) to adopt a strategy of inclusive "racial assertiveness." Unlike the earlier Africanism of the Pan-Africanist Congress, which emphasized the exclusive racial unity of Africans, the BCM developed a broader notion of blackness. Black did not merely refer to Africans, it also encompassed Indians and coloreds. Blackness thus embraced all oppressed South Africans, irrespective of race.

Marx rightfully emphasizes that the more inclusive stance of the BCM did not imply "multiracialism," for whites were not welcome to join the movement. As he explains, the "everyday experiences" of the advocates of black consciousness "reinforced the idea that oppression was linked to race" (p. 43). It was only when blacks had achieved unity and a sense of their humanity that whites would begin to think about surrendering their power. According to Marx, the immediate goal was black psychological emancipation. The struggle against white supremacy was thus waged at the level of ideas. Supporters of black consciousness were convinced that apartheid would crumble once blacks had rejected their mental enslavement to white supremacy, "regardless of the intentions of those in power, or the balance of material forces controlled by the state and its opponents" (p. 47).

By the mid 1970s, the focus on psychological emancipation had fostered a widespread culture of black defiance, particularly among the township youth. Submissive quiescence to apartheid gave way to an acute sense of moral outrage. Marx persuasively argues that such anger, combined with the deterioration of black living standards, led to the Soweto revolt of 1976. While the revolt failed to dislodge the white minority from power, it indicated a turning point in South Africa's history.

The Soweto uprising marked "the beginning of a decline in the mass influence" of the BCM (p. 72), as all hopes of using the sole weapon of psychological liberation to overthrow apartheid vanished under the weight of state repression. The BCM's eclipse and fragmentation facilitated the political ascendancy of the African National Congress (ANC) and trade union organizations. Marx thoughtfully argues that the emergence of these movements prompted a redefinition of identities to suit new forms of struggle and generated new ideological constructs.

While the BCM—reorganized in 1978 under the name of the Azanian Peoples Organization (AZAPO)—embraced socialism and the concept of class, it remained deeply committed to the "ideals of racial identity" (p. 88). As Marx remarks, AZAPO defined blacks "as a 'race of workers,' thereby collapsing the class category of workers into the racial category of blacks" (p. 87). According to the author, AZAPO became so absorbed in doctrinal debates that it failed to concentrate on the more crucial task of organizing a mass following. By the early 1980s, the search for doctrinal purity transformed the once-dominant BCM into a relatively weak political organization. Taking advantage of this weakness, the ANC in exile successfully imposed its hegemony over the internal opposition movement in South Africa.

Marx's explanation of the rise of the ANC is well argued and convincing. He shows how the exiled leadership's patience and discipline contributed to make the organization into the symbol of African opposition to apartheid. The ANC was greatly helped by Nelson Mandela, whose long imprisonment only enhanced his already mythical stature at home and abroad. Moreover, the ANC developed an international network of support that enabled it to acquire the material resources with which to absorb thousands of young exiles who wished to join Umkhonto we Sizwe—the movement's armed wing. While Umkhonto's violent resistance never posed a serious military threat to the white state, it greatly increased the ANC's prestige and popularity. Material and organizational resources were thus decisive in establishing the supremacy of the ANC over the internal opposition. Yet material resources by themselves, Marx argues, do not fully account for the movement's phenomenal success. Clearly, other factors came into play.

Chief among them was the ideological convergence of the ANC with the United Democratic Front (UDF). Founded in 1983, the UDF "grew out of local civic and professional groups" that had strong connections to the ANC (p. 14). These connections rapidly led the front into adopting the fundamental tenets of the ANC's seminal Freedom Charter. The charter purposefully "eschewed absolute ideological coherence" to embrace a flexible and pragmatic vision of a multiracial struggle against apartheid (ibid.). The goal was to forge a nonracial national identity that would serve as the basis of the "most inclusive possible unity" of all forces opposed to white supremacy (ibid.). As Marx contends, "Not only did such a national identity avoid the exclusion of any race or class, but it also was elastic enough to accommodate the broadest possible array of groups and ideas" (p. 121).

Thus, the ultimate aim of "charterism" was to offer an alternative social order that would be acceptable to all except virulent white supremacists. A "historic compromise" ushering in "majoritarian rule with various checks" (p. 262) became virtually inevitable when, in 1990, President De Klerk unbanned the ANC and accepted the negotiated demise of apartheid.

Marx does not imply that the road to a democratic postapartheid society will be easy. He acknowledges the difficulties stemming from the very idea of a "historic compromise." Among radical ANC supporters, particularly in the Congress of South African Trade Unions and the South African Communist party, there are clear apprehensions that an "accommodation among elites" (p. 262) will not resolve the acute divisions and injustices of class and that the promise of a socialist South Africa will give way to a pragmatic acceptance of capitalism. Among conservative Africans, such as the Zulu chief Gatsha Buthelezi, there is fear that the triumph of the national identity espoused by the ANC will undermine their tribal bases of support and thus confine them to political oblivion. For Marx, it is this fear (not ethnic enmity) that has generated the escalating violence between African supporters of the ANC and anticharterist adherents of Buthelezi's Inkatha party. Finally, the author does not dismiss the dangers of a right-wing white backlash degenerating into indiscriminate vigilantism.

Marx, nevertheless, remains optimistic, highlighting the fact that "most South Africans have remained committed to the formation of a nonracial society, in which races are not merely accommodated but cease to be divisive" (p. ix). The author's assessment seems warranted. Since the publication of *Lessons of Struggle*, negotiations have led to a major agreement for multiracial elections and majority rule. Thus, even if South Africa's future is as yet uncertain, there is clearly reason for hope.

The author's cautious optimism is grounded in his thorough familiarity with the South African terrain. With that knowledge, Anthony Marx has produced the most comprehensive and informed account of black South African opposition to apartheid for the 1960–90 period. *Lessons of Struggle* is a must read for anyone interested in social change, for its insights apply beyond South Africa. Marx's outstanding study is timely, intellectually rewarding, and thought-provoking. It deserves a wide audience.

University of Virginia ROBERT FATTON, JR.

Encounters with the Contemporary Radical Right. Edited by Peter H. Merkl and Leonard Weinberg. Boulder: Westview, 1993. 277p. $65.00 cloth, $19.95 paper.

This book provides an insightful account of the current state of the radical Right in numerous countries. The collection of essays, which are loosely organized according to region (Western Europe, Eastern Europe/Israel, Anglo–American Democracies) clearly transcends the Eurocentrism or American (German, British, etc.) "exceptionalism" that often underlies the approach to this subject. Taken together, the various contributions demonstrate the almost global dimensions of the phenomenon of right-wing extremism and support the view that we are dealing with a "normal" pathological condition in modern dynamic societies.

But the value of this volume is more than its geographic reach. It raises basic conceptual questions concerning underlying assumptions in the analysis of the radical Right. Thus, Leonard Weinberg, in the introduction, points out the confusion in the use of various terms (extreme Right, radical Right, fascism) and offers a working definition that starts with a concept of political extremism, a basic differentiation of left and right, and the identification of themes common to the radical Right, such as nationalism and ethnocentrism (not necessarily anti-Semitism), the use of conspiracy theories, and populism (pp. 7–8).

Moreover, the role of historical continuities and contexts (esp. the relationship of the contemporary radical Right to historical fascism) is given considerable weight in the book. For example, in an extremely informative and comprehensive examination of the French Front National (FN), William Safran provides a valuable interpretation of the FN's electoral performance and support patterns while at the same time analyzing similarities and differences between Le Pen today and Hitler in the 1920s and between the Poujadist movement and the FN. In a similar vein, Eckart Zimmermann and Thomas Saalfeld distinguish three waves of right-wing extremism in West Germany from the end of World War II to unification and suggest more discontinuities between the last wave of the 1980s and its predecessors than between the first two waves and the Nazi regime. Likewise, Trond Gilberg, in his contribution on Romania, discusses to what extent historical legacies of interwar fascism and the Ceaucescu regime (e.g., extreme nationalism, ethnochauvinism, agrarian populism, and native fascism) provide the basis for current expressions of the radical Right and impose limits on democratic renewal in contemporary Romania. The theme of historical continuities is taken a step further in Peter Merkl's conclusion, which summarizes and synthesizes many of the individual chapters' findings and includes comparisons with countries that are not even covered by the contributions to the book—particularly, East Germany before and after unification.

The book, however, is very uneven. For one, the quality of the individual contributions varies. None of the other essays matches the rigor and the richness of Safran's analysis. Another problem is that part of the book is outdated. The time-boundedness of some of the essays shows most clearly in the case of Vladislav Krasnov's study of the Russian Pamiat. While it is very important to have a look at Russian nationalism, especially in the light of recent developments, it is surprising that the chapter focuses only on Pamiat. There is little anticipation of the Zhirinovsky phenomenon (except in Merkl's perceptive conclusions) and much reliance on the old Kremlinological approach of interpreting Soviet politics via (Pamiat) documents and official statements, rather than through a more structural analysis of the place and power of right-wing elements in Soviet (and post-Soviet) political life. Similarly, Zimmermann and Saalfeld's conclusion on the waning relevance of Schönhuber in the face of the "organizational chaos on the political right" and the "good long-run economic forecast" (p. 73) is refuted by developments since 1990 in Germany. It could be argued, of course, that these developments were unpredictable. However, a different focus (i.e., less on economic factors and more on changes in culture and social structure) would have enhanced the capacity of the study to withstand the test of time. It is also curious that Zimmermann and Saalfeld make so much of the role of conservatives in power. This does not apply to France, the United Kingdom, or the United States.

But beyond these and other individual shortcomings, the book suffers a more basic problem of unevenness. The criteria for defining the radical Right across countries, for example, changes from chapter to chapter. While some of the contributions examine parties and electoral trends on the far Right (and Safran goes even further in looking at the much-too-often-neglected links between the radical Right and established parties), others focus on fringe groups, right-wing militant behavior, and violence outside of the "mainstream." Thus, it is questionable whether it makes much sense to focus on the American radical Right in terms of hate groups and Nazi organizations (as Weinberg does), while excluding the New (Christian) Right and the larger context of racism and right-wing orientations in the American public. This fails to do justice to Weinberg's own set of criteria laid out in the introduction. It also contrasts sharply with Merkl's inclusion of the New Christian Right in the concluding summary, as well as with the level of analysis informing other chapters.

Finally, while raising some interesting theoretical and conceptual questions, this book nonetheless neglects much of the basic (as well as the more recent) scholarly work on the subject. Apart from Zimmermann and Saalfeld's effort to discuss some of the concepts of right-wing extremism in the light of the (West) German evidence, there is hardly any attempt (e.g., in the introduction or the conclusion) to provide a more theoretically informed discussion that would integrate the considerable cross-country evidence presented in this book with Theodor W. Adorno's, Seymour M. Lipset's, or Ernst Nolte's work or with any current theoretical literature.

The book makes a significant contribution through cross-country surveys of the modern far Right. The editor's claim to provide a book with an "unusually wide scope and comparative thrust" (p. 11), however, is only half met. A genuinely *comparative* study of the contemporary radical Right has yet to be written.

Cornell University MICHAEL MINKENBERG

Public Opinion and Regime Change: The New Politics of Post-Soviet Societies. Edited by Arthur H. Miller, William M. Reisinger, and Vicki L. Hesli. Boulder: Westview, 1993. 310p. $44.95.

Russia is a work in progress, and so, in many ways, is the subject of this book. The three surveys that form the central core of most of the chapters were fielded in various parts of the Soviet Union in the spring of 1990 and, for a section on Estonia, the winter of 1991. The analyses may thus be seen as a useful first pass at the unfolding story of Soviet and post-Soviet public opinion.

The authors address their studies to readers who are not specialists in the politics and economics of the region, and they organize the sections thematically, treating the organization and methods of public opinion research in the Soviet Union, the individual and politics (political tolerance, participation, political culture, and integration), and attitudes relating to economic questions and foreign policy issues. The surveys and about a third of the chapters are welcome results of the collaboration between Soviet and American scholars in survey research and analysis. The editors contribute the introduction and conclusions, Michael Sacks a foreword, and Thomas Remington, an afterword.

While generally successful, the editors' thematic organization for nonspecialists poses a difficult task, since each contribution tends to repeat both contextual information necessary to understand the Soviet issues of the time and relevant dimensions of the surveys that most of the authors draw on and that are described more fully in the appendix. Though sometimes too thin, these introductions can be masterful examples of comprehensive concision, as is the case with the chapter on center–periphery relations by Vicki Hesli and Joel Barkan.

Jeffrey Hahn's "Public Opinion Research in the Soviet Union: Problems and Possibilities," provides a checklist for Westerners new to the enterprise to evaluate Soviet survey research and is still to the point, even as new survey organizations mushroom and new sampling methods are designed (though often undercut by rapidly deteriorating conditions of public safety and transportation). Mapping public opinion on various issues is also an important contribution. Cynthia Kaplan, in "New Forms of Political Participation," gives a nuanced overview of traditional and unconventional forms of political participation.

Although the surveys reported here do not yet link attitudes to voting behavior (with a very limited exception), they do illuminate sources of support for reform, both political and economic. James Gibson and Raymond Duch, in "Emerging Democratic Values in Soviet Political Culture," examine tolerance on a number of dimensions, including willingness to permit rights for disliked groups, and are optimistic about what they see as an emergent democratic political culture. A valuable addition to this discussion would have been an analysis of the varying degrees of perceived threat these disliked groups posed for respondents—a part of the tolerance debate that becomes increasingly important over time and across periods of confrontation and civil strife.

Gibson and Duch find (as do Arthur Miller on regime legitimacy, William Zimmerman on attitudes toward foreign policy, and the SIP émigré survey and earlier Soviet surveys) that the great achievement of the Soviet era—near universal literacy and at least secondary education—served to make the young aware of alternatives and dissatisfactions, rather than socializing them in the norms of the system. Interestingly, three Russian contributors are considerably more pessimistic about the irreversibility and depth of attitude change. Gennady Denisovsky, Polina Kozyreva, and Mikhail Matskovsky find that overall, no more than a third of the respondents displayed the value orientation necessary to manage a difficult transition to the market economy. Reisinger and Nikitin point to party formation as a key component of the transition process and warn, on the basis of more current observations, that the inability of reform leaders to mobilize supporters may lead to electoral rejection of reformist leadership, as several recent cases in the region have demonstrated.

Several of the authors look at a changing pattern of confidence in political institutions. Andrei Melville positively assesses what he finds to be a growing confidence in new institutions. Plumbing regime support during regime change presents difficulties, not least of which is the identification of an institution as "new" or "old." The Supreme Soviet (and its parent, the Congress of People's Deputies) was, by the time of the May–June 1990 survey that Melville reports, a reconstituted, newly elected body, whose contentious and freewheeling debates were broadcast to a spellbound television audience. At the time, respondents in other surveys considered the institution—if not "new"–at least "renewed" and not "old," as the Party, military, and youth organization were. Melville's examination of support for new protoparties also needs to balance a favorable conclusion about such responses with the large number of people who evidently did not answer or, perhaps, recognize these groupings. A systematic analysis of the *don't know* response, such as that given by Gibson and Duch would have been helpful throughout the book.

Underlying nearly all the contributions is the search for trends. Since the research reported is not longitudinal, evidence of trend is extracted from comparisons with other surveys (Soviet and émigré) or respondents' retrospective evaluations (John Willerton and Lee Sigelman's "Perestroika and the Public: Citizens' Views of the 'Fruits' of Economic Reform"). Several of the authors have, since publication of this book, replicated their earlier surveys and have produced direct analysis of trends.

A discussion of ethnicity is central to several chapters of the book. At this early stage, ethnicity, though it figures importantly, is related primarily to titular nationality alienation from central Soviet power in selected non-Russian republics. But ethnic issues proved to be far more durable and complex. The discussions by Hesli and Bracken and Lois Sayrs and James Lindsay attempt a finer-grained analysis by comparing the attitudes of titular and nontitular ethnic groups within a given region; but, as is often the case, rather few of the latter fell into the sample, making it difficult to generalize from, say, the Russians in West Ukraine in the sample or the unspecified but undoubtedly small numbers of Poles and Jews. However, readers charting the subsequent course of communal politics will be usefully reminded, by these studies, of the initial sources of ethnically based disaffection.

The public opinion industry in the former Soviet Union has certainly been a growth sector, but the December 1993 Russian parliamentary election had a sobering effect. Between the period of the surveys reported in this book (a year-and-a-half before the Soviet Union dissolved) and 1993, when Vladimir Zhirinovsky, a candidate of obscure origin and overweening self-promotion, rode a wave of deep, underestimated discontent to Russian parliament, attitudes, as well as the environment, had changed. As the studies in this book and later surveys make clear, there is little sentiment in Russia for reinstating noncompetitive elections (and it should be recalled that by no means all post-Soviet societies have replaced them); but the fragility and constant fission characterizing political parties make them a critically weak link in the political process.

Duke University ELLEN MICKIEWICZ

The Culture of Conflict: Interpretations and Interests in Comparative Perspective. By Marc Howard Ross. New Haven: Yale University Press, 1993. 240p. $30.00.

Conflicts become acute not only because of what is at stake but because of the social and psychological consequences of winning and losing. Anthropological evidence indicates that societies develop characteristic patterns of disputing that determine which groups and individuals fight, what they fight about, how they fight, and how those fights are resolved. Cultural patterns determine the intensity of conflict in any society and the extent to which that conflict is directed against outsiders or members of the society. Cultural orientations toward conflict are developed and transmitted through social relationships. Early childhood experiences build (or fail to build) trust, security, and efficacy toward self, others, and the social environment. Institutions and practices reinforce these psychocultural dispositions. They sanction certain values and behaviors, determine group identity, and define aggression and appropriate responses to it.

Using data from a sample of 90 preindustrial societies, Marc Ross develops and tests hypotheses about the relationship between cultural norms and the intensity of conflict. He finds that internal and external conflict are higher in societies where socialization is relatively low in warmth and affection and harsher and higher in male gender-identity conflict. Conflict is correspondingly lower in societies that express warmth and affection and encourage the development of unconflicted gender identities. In peaceful societies, overt expression of aggression is often discouraged by quickly removing the would-be offender from the situation. In such societies, children have multiple, affectionate caretakers, there is continual surveillance of behavior, gender differentiation and sexual tension are low, and sharing is highly valued. Internal images of others tend to be relatively supportive, trusting, cooperative, and responsive. When these expectations are reinforced by behavior, the pattern of low conflict becomes self-sustaining.

Ross also acknowledges the role of structure and identifies two dimensions that are relevant to conflict behavior. The first is the extent to which people with common interests in one domain share them in another. When social organization reinforces a single, dominant cleavage, conflicts escalate because there are few common interests to restrain them. When the lines of cleavage are many and cross-cutting, the parties are likely to have closer ties and more common interests and thus more incentive to contain their conflict. The second dimension is the level of socioeconomic development and political complexity. Development increases internal conflict because it brings more resources, military capability, and inequality; but political complexity can limit this conflict through the peacemaking role of the state. The data are less ambiguous with respect to external conflict: more complex societies have higher levels of external conflict.

In preindustrial societies (as in modern states), internal and external conflict are positively correlated. Ross contends that both kinds of conflict have similar psychocultural causes: low warmth and affection, harsher and higher male gender-identity conflict. The structural causes vary. In societies with strong cross-cutting ties, hostility is more likely to be directed against outsiders. In societies where there is a single, dominant cleavage, little differentiation is made between internal and external targets.

Ross argues (and I agree) that the psychocultural approach is germane to understanding contemporary conflicts. Modern states are admittedly very different from the small, traditional, preindustrial societies that largely comprise the data base for the study; but they share important social and psychocultural dynamics. This said, Ross's attempt to the explain the relative tranquillity of Norway and the violence of Northern Ireland in psychocultural terms is unpersuasive. Proponents of purely structural explanations have a ready one at hand: Norway is peaceful because it is ethnically and economically homogenous, and Northern Ireland violent because of acute sectarian and class divisions. Moreover, any argument that explains Northern Irish violence in terms of the culture of the inhabitants, Catholic and Protestant, cannot explain the relative peacefulness of the Republic of Ireland, whose citizens share the same culture. A more convincing demonstration of the psychocultural thesis would have utilized cases that appear anomalous and difficult to account for, for example, a violent, homogenous society or one that is peaceful but ethnically divided or economically stratified.

Ross has nevertheless written a very important book. He has identified cultural traits associated by violence and social structures that determine its focus. His findings indicate that conflict management must be tailored to the nature of the society and that the success of different strategies will depend, in the first instance, on the nature of culture in which they are applied. This is the subject of a companion volume, *The Management of Conflict*. Ross also provides a powerful demonstration of the influence of culture on the definition of interests and the choice of means to advance and defend them. His evidence indicates that interest-based rational choice arguments that fail to build on culturally determined conceptions of interest and rationality will have only limited utility.

University of Pittsburgh RICHARD NED LEBOW

The Political Logic of Economic Reform in China. By Susan L. Shirk. Berkeley: University of California Press, 1993. 399p. $48.00 cloth, $15.00 paper.

Susan Shirk's new book will long remain a classic account of the policies that revitalized China's economy during the 1980s. Drawing on extensive interviews with Chinese officials, primary sources, and a rich variety of social science theory relevant to her analysis, Shirk explains how and why the political institutions established in the era of Mao Zedong did not prevent but, instead, shaped (and perhaps ironically may have facilitated) China's reforms.

Shirk's essential argument is that a political logic, rather than an economic logic, best accounts for the policies adopted to reform the key state industrial sector of China's socialist economy. Unlike Mikhail Gorbachev, who sought support among the general population as a counterweight to the vested interests of Soviet central planning, Deng Xiaoping and his associates built a coalition of support for reform within a portion of the existing system's political elite, mainly provincial party officials. The political consequence of this successful strategy to establish the dominance of the reformist line was to strengthen the power of provincial leaders of the communist party. The economic consequence was that reform policies emerged as "particularistic contracts" providing mutual benefits for local leaders and contending central elites in a mixed socialist–market economy. Pork-barrel politics, Chinese style, resulted in a "second-best" outcome of extensive (as opposed to efficient, intensive) growth. Uniform, impersonal rules of the game for actors in a fully marketized economy, though perhaps economically optimal, were not politically viable.

The book is presented in three parts. Part 1 contains a broad overview of the argument and research design, as well as a brief background analysis of China's ambitious reform program. Part 2 presents a masterful study of the Chinese communist political institutions on which Shirk focuses to explain the path of economic reform in China's state industrial sector. Five chapters cover authority relations (the principal–agent relationship between Communist party and state bureaucracy), leadership incentives (rooted in reciprocal accountability between leaders and members of the elite selectorate), bargaining arena (organizational setting), enfranchisement (representation of sectoral and regional interests), and decision rules (delegation by consensus). A sixth chapter integrates the theory with an analysis of China's economic reforms. Together, these chapters could easily stand alone as required reading in any comparative politics course. Though some may find specific points with which they take issue (especially the weight assigned to the Central Committee, as opposed to the elders, military, and preeminent leader in the elite "selectorate"), Shirk's argument is elegant, is compelling on its own terms, and demands to be addressed. Her institutional analysis provides a persuasive explanation of the pace (gradual), sequence (postponing the difficult, redistributive choices), content (dangerously expansionary), and form (particularistic contracts, rather than uniform rules) of the policies that China embraced during the 1980s.

In part 3, Shirk fleshes out her argument by tracing the history of several key economic policies, most notably the fiscal decentralization known as "eating in separate kitchens" (that granted greater economic power to provincial leaders) and the attempts to recapture lost central revenues by substituting uniform tax policy for the method of negotiated contracts to hand over a share of enterprise profits to the state. These case studies demonstrate how the incentives established by China's political institutions facilitated reforms that allowed central leaders to dispense favors to potential supporters among the provincial elite (e.g., "eating in separate kitchens," profit contracting), and usually precluded those that harmed provincial interests or deprived central leaders of control over political pork (e.g., a universally applied tax code). Although this might sound like mighty dry territory, the story remains fascinating because Shirk's prose is as elegant and understandable as her theoretical argument. She deftly highlights the link between these economic policy debates and the palace intrigue among aspiring successors to Deng during the 1980s.

One of the strengths of Shirk's argument is that it clearly establishes a basis for testing her hypotheses about policy and political incentives against evidence emerging in the period after that covered in her study. Vested provincial interests in half-reform based on particularistic contracting helps explain the limited ability of conservatives and central planners to exploit the political crackdown following June 1989 to roll back the basic reforms. Shirk's analytical framework also illuminates the enthusiastic response by local officials to Deng's 1992 call for the resumption of high-speed growth and provides a perspective for interpreting the renewed attempts beginning in 1993 to deal with the negative economic consequences of the type of reforms feasible in the 1980s. China's recent drive to increase the efficiency of state-owned enterprises, to create effective macroeconomic tools for the central government, and especially to rationalize the collection of tax revenues are part of a package intended to move from a system where particularistic arrangements based on bargaining and political interest prevail to one in which uniform rules are set and economic actors compete. But as Shirk puts it, comprehensive reforms entail painful redistribution and challenges to vested interests. This is possible "only if the government has the financial wherewithal to buy off those who stand to lose by the policy or if the party leadership is united and strongly committed to the policy" (p. 309). Given strained central resources and the ongoing succession struggle for the leadership of post-Deng China, Shirk's analysis underscores the political-institutional obstacles to economically sensible comprehensive reform confronted in the mid-1990s.

In a book covering such a broad range of important issues, it is inevitable that readers will find points with which they disagree. One of Shirk's more controversial assertions is that the tendency of leaders to "play to the provinces," though economically suboptimal, was not politically dangerous. She argues that the central party apparatus' powers of appointment and resource allocation mean "the center has little reason to fear that delegations of fiscal authority to the provinces will lead to a loss of central control or national disintegration" (pp. 149–50). However effective these levers of power may now be, their durability in China's changing socioeconomic context is questionable.

A second concern for some will be that Shirk's selective focus (appropriate in the development of a rigorous theoretical argument) necessarily omits—or gives short shrift to—a number of important influences on the reform process. For example, focusing on the incentives and constraints established by the roles and institutions of the Chinese communist polity, Shirk recognizes that she can refer to but cannot fully incorporate the personal power wielded by Deng Xiaoping (and other leading individuals). Yet there can be little doubt about Deng's distinctive influence in establishing the overall goals and direction of reform and his ability to intervene at critical junctures (e.g., endorsing special economic zones, initiating ambitious price reform in 1988, and most notably reviving aggressive marketization in early 1992).

Readers may at times also disagree with Shirk's emphasis on the connection between political-strategic calculations and the substantive policies advocated by key leaders. Why, for example, did Deng choose to support the initiation of market reforms in the late 1970s? Shirk emphasizes the power-political, rather than economic, rationale for Deng's position, arguing that it was his strategy for wresting power from Mao's designated successor, Hua Guofeng. Shirk is right to remind us that power concerns, as well as policy concerns, must be analyzed. But it is not clear that Deng needed to embrace the unorthodox policies he did in order to defeat Hua. Given Hua's mishandling of the economy, Deng could have eased his rival aside simply by claiming greater competence as a leader and by drawing on his "broader and deeper" base of power, to use Lowell Dittmer's phrase. Deng's decision to initiate market reforms may be better explained not by power politics but by his substantive policy preferences (reflecting lessons he had been drawing about the Chinese experience with socialism since the collapse of the Great Leap Forward), the sort of consideration that Shirk acknowledges but that necessarily remains outside her analytical framework.

Some readers may wish that Shirk had not limited her empirical focus to the reform of state-owned industry, especially as it is clear that by the 1990s, the country's center of economic gravity was shifting to the private, collective, and foreign joint-venture sectors (a point Shirk acknowledges). But the economic importance of state-owned industry during the 1980s, the continuing political power of its representatives in the central leadership, and its ideological meaning for a regime determined to retain the fig leaf of socialism even as it establishes a market economy all suggest the significance of the issues Shirk addresses. Moreover, the elite dynamics she highlights in a bureaucratic–authoritarian regime of the communist type, may, with modification, prove useful for understanding the opportunities and constraints facing China's leaders as they tackle other sectors of the economy.

Susan Shirk's book is essential reading for those claiming to understand the reform era in China, the evolution of communist regimes, and the dynamic interaction of politics and economics. Her work not only casts fresh light on the recent past but also establishes a theoretical perspective that will be at the center of scholarly debate for years to come.

University of Pennsylvania AVERY GOLDSTEIN

Cages of Reason: The Rise of the Rational State in France, Japan, the United States, and Great Britain. By Bernard S. Silberman. Chicago: University of Chicago Press, 1993. 487p. $65.00 cloth, $19.95 paper.

Analyses of public bureaucracy have tended to fall into two categories. Theoretical approaches, such as Max Weber's ideal type and economics-based models dealing with budget maximization, slack, and agentry shirking, often posit a high degree of uniformity in bureaucratic structure and/or behavior. Empirical studies, by contrast, reveal great variety in the organization, behavior, patterns of corruption, and cultures of national bureaucracies. Thus, rather than converging as much theory would predict, public administrative rationalization has resulted in different outcomes. By deftly analyzing national administrative development in France, Japan, the United States, and Great Britain, Silberman seeks to build a theory explaining such variation.

Silberman's thesis is that administrative "rationalization took only two courses. One . . . was characterized by the location of skills, information, and their regulation in the individual per se through the process of professional training . . . The second basic type . . . is charcterized by organizational control and allocation of information and skills as well as by organizational surveillance and control of information usage" (p. x). These two forms of rationalization create different organizational and political dynamics. Professionals are apt to have a public service orientation, engage in self-regulation, and be able to practice their professions in a variety of organizational settings. Organizationally oriented bureaucracies have much tighter career and control structures. Here, individuals must commit early to careers in the public service, lateral entry is unusual, and the opportunity for training is heavily regulated by the bureaucracy itself. Organizationally oriented bureaucracies have stronger departmental identities and cultures, are less permeable, and are more difficult for outsiders to control.

Leaving aside the question of whether these really are the only two courses available, why would a polity opt for one or the other? Silberman argues that "the variation can be correlated to historically different experiences with regard to the problem of [political] leadership succession in the nineteenth century" (p. 413). Where there was high uncertainty regarding succession processes and outcomes, as in France and Japan, organizationally oriented administrative rationalization reduced the risks faced by new leaders seeking to solidify their power, authority, and legitimacy. In low uncertainty circumstances, as in the nineteenth century United States and Great Britain, clear and systematic rules govern leadership succession. The problem for political leaders under low uncertainty is to rationalize administration without losing control over the distribution of public goods. Professionally oriented bureaucracies serve nicely because they reduce corruption while seeming incapable of exercising great independence or autonomy.

Silberman further maintains that within each of these overall patterns of rationalization there is variation depending on the character of the leadership structure. Leadership based on social networks results in bureaucratic domination under conditions of high uncertainty and bureaucratic consensus under low uncertainty. Leadership based on party structure results in either single (high uncertainty) or general party domination of administrative posts.

Silberman's framework substantially advances theory regarding variation in national administrative rationalization and should be used to inform additional studies. It will be particularly interesting to learn whether other courses of bureaucratic development are available. It is not clear why Silberman's types are either exhaustive or dichotomous, unless his contention is merely definitional—either bureaucracies have tight career structures or not, and, if not, are "rational" only if professionally oriented. Even then, hybrids would seem possible. The extent of professional or organizational orientation need not be constant across all agencies in a national bureaucracy. Policy arena and administrative function may often be more useful units of analysis. Because Silberman's analysis focuses more on the formal qualities of bureaucracy and law, it does not say much about diversity in administrative practices, organizational cultures, and corruption within and across national bureaucracies. Further, as Silberman argues, leaders face high and low uncertainty regarding succession in different social, economic, and legal contexts—and there are many additional contexts to be explored. All this said, Silberman has produced an invaluable, densely packed work that those with deep knowledge of public administrative development will find extremely rewarding.

American University DAVID H. ROSENBLOOM

INTERNATIONAL RELATIONS

Revolution and World Order: The Revolutionary State in International Society. By David Armstrong. New York: Oxford University Press, 1993. 328p. $59.00.

Revolutionary states, by their very nature, deliberately have placed themselves in a posture of confrontation with international society. The mutual relationship between such revolutionary states and the Westphalian international order is the central theme of David Armstrong's new book. It is a compelling account of how states that have fundamentally rejected the premises of the international society of states over time come to adapt themselves to that system. But it is also a rendering of how conceptions of international order mutate (at the margins) to accommodate the powerful ideas which provide the impetus of revolution. This study asks, To what extent do revolutionary states succeed in altering international society? To what extent do they become "socialized"? Finally, to what extent is international society possible in a world where some states deny the very existence of common rules, interests, and institutions?

Armstrong explores these questions by reviewing the ways in which revolutionary thought in America, France, Russia (and, in less detail, Libya, China, Cuba, and Iran) clashed with prevailing premises of the "Westphalian conception of international society." In various ways, he argues, revolutionary ideas have challenged traditional notions of state sovereignty and sovereign equality. Popular sovereignty, class struggle, and religious principles have fueled revolutionary challenges to the view of the state as the source of all legitimate authority. Revolutionary states have often questioned the validity of positivist international law and some have rejected its most central tenet, that of nonintervention in the domestic affairs of other states. The consequences for international relations are potentially stark.

The challenge to state-centric, positivist international order is a theme that runs through the study. Jefferson's belief that the underlying principles that should govern the behavior of states (as well as individuals) were to be derived from natural law demanded a standard of morality in the conduct of international affairs that sat uneasily alongside the power politics of eighteenth-century Europe. French revolutionary thought proposed that sovereignty is invested in the people, not in the ruler, and advanced a novel principle of international legitimacy that threw into question state consent as the basis of international obligation. Perhaps the most thoroughgoing rejection of the traditional conception was the Bolsheviks' historical materialism that viewed the true motive force of history as the struggle between classes. The transnationalism inherent in the claim that "working men have no country" constituted a comprehensive denial of the state itself—the basic unit of the Westphalian structure. A number of parries against the traditional international legal system have emanated from the Third World in the twentieth century (China, Indonesia, Cuba, Libya, and Iran); and while these have disparate geneses, they have in common the tendency to view world politics as a struggle between good and evil, to reject the notion of any integrated international society, and to deny that they are bound by established rules of international law.

Armstrong weaves a subtle description of the interplay between the philosophy, the politics, and the diplomatic practices of each of these revolutionary states. In the case of the United States, France, and the Soviet Union, he is especially careful to present the texture of the domestic debate and political struggles that underlay the formation of the revolutionary state's posture toward established international society. In the American case, for example, Jeffersonian universalism, which Armstrong describes as "moralistic," even "self-righteous," was in time counterbalanced by Hamilton's views that in order to participate in international relations on equal terms with the European powers, it would be necessary to adapt American ways to theirs. Hamilton's thinking therefore represented an attempt to bring about the socialization of the United States within the prevailing international society, though Jefferson's universalism is clearly discernible in the various moral crusades in which the United States has taken part in this century.

An interesting subtheme in this work is how and why international socialization itself takes place. Armstrong writes that "the existence of the international society is a reality with which all states, however unique, have to come to terms through a socialization process that is common to all new entrants" (p. 77). In the American case, the need for international legitimacy and respectability was the driving force for socialization; the primary American goal, after all, was to become a state. The revolutionary French found that their claims to a unilateral right to set aside treaty obligations regarding the Schelde River, as well as their assertion of some nebulous "general will" to justify interference in the domestic politics of other states, increasingly made it difficult to conduct foreign relations with the British. Even Lenin, whose Marxist philosophy denied the ultimate legitimacy of the state at all—not to mention an international "society" of states—had to come to grips with the political reality that the "socialist fatherland" was surrounded by imperialist states perceived to be bent upon its destruction. During the interwar years, a schizophrenic Soviet diplomacy reflected the perceived need for allies in the international labor movement, as well as the very real imperative to carry on relations with other states.

The relationship between revolutionary states and international society is hardly unidirectional, however. Each of the cases presented in the book displays a high degree of socialization—strong testimony to the lure of state-centric international law for those who would grasp the reins of state power. But over time, the Westphalian order has had to adapt in some respects to the ideas of revolutionary states. As Armstrong notes, "When other ideas about the role of the state—that it should embody the 'nation', that it should be responsive to the will of the 'people', that it was the standard bearer of 'history', that it was responsible for the welfare of its citizens or of other states' citizens—began to take hold, there were inevitable consequences for the society of states" (p. 40). The French Revolution, for instance, introduced the idea that *peoples* or *nations* might have some standing in international law and, indeed, might have a strong legal claim to having some say in their state affiliation or even a claim to a state of their own. Not only has this idea been a driving force behind decolonization after World War II, it has been cited to justify boundaries as multinational empires crumbled in the 1910s and again in the 1990s. The French and American revolutions spawned the essential republican ideas that underlie international law's relatively recent concern with human rights, most especially those contained in the International Covenant on Civil and Political Rights. The American rejection of traditional European diplomacy and the use of force based on the balance of power can be viewed as a precursor to Wilsonian collective security, in which any unlawful international aggression might be openly debated and, hopefully, either deterred or arrested collectively. Even the Bolshevik Revolution has had its mark on international law: the traditional requirement of *pacta sunt servanda* (treaties must be served) has since been widely accepted to be qualified by the subclause *rebus sic stantibus*, which expresses the principle that treaty obligations may be invalidated by a fundamental change of circumstances, such as, the Soviets have argued, socialist revolution.

Nonetheless, one is impressed more by the durability of the Westphalian model than by its malleability. Despite the fact that "the Westphalian conception of international society found itself squeezed between two powerful alternative visions which had their origins in two very different revolutionary experiences" (p. 75), the challenge to the system appears to have been more at the margins than at its core. As Armstrong notes, state-centered international society endures and continues to be accepted by states "because the foundation of their association has been the common defence of sovereignty" (p. 311). This book successfully weaves together the political, legal, and philosophical threads of revolutionary challenges to the enduring fabric of international society and is an important contribution to our understanding of the mutual relationship between the revolutionary impulse and the conservative pillars of international society.

Duke University BETH A. SIMMONS

Breaking the Ice: Rapprochement between East and West Germany, the United States and China, and Israel and Egypt. By Tony Armstrong. Washington: United States Institute of Peace Press, 1993. 187p. $13.95 paper.

This slender volume is a welcome addition to the growing literature on the causes of peace. Employing the method of "structured, focused comparison," Armstrong examines three cases of *rapprochement* between states with a long history of hostile relations. From these he extracts five "principles of icebreaking," which he uses to shed light on the prospects for reconciliation between some present-day adversaries.

Unlike most previous studies, *Breaking the Ice* attempts to identify both structural factors (circumstances) and process factors (strategies, tactics) conducive to rapprochement. Armstrong's intent is to develop theory that is not only explanatory but policy-relevant. Hence, he focuses mainly on process (which is more susceptible to manipulation by policymakers), and four of the propositions he derives from his case studies are couched in prescriptive language. Among these findings are that "major issues should be fractionated in negotiations aimed at improving relations" and that negotiations "should be conducted at a high level . . . and should be nonpublic" (p. 134). Armstrong admits that his conclusions "contain no great surprises; indeed, they embody much that is common practice and appears to be common sense" (p. 134). Still, by confirming the validity of assumptions that had not been systematically tested, he has performed a valuable service to theorists and policymakers alike.

Studies of rapprochement, like those of détente and related phenomena, encounter certain conceptual difficulties. Is rapprochement a *process*—a movement along the continuum between war on the one hand and stable peace (security, community) on the other? Is it also a *condition*, a specific type of relationship resulting from such movement? If so, what are its essential characteristics?

Armstrong operationalizes rapprochement as the normalization of relations, that is, mutual diplomatic recognition and the exchange of ambassadors. This definition provides a simple criterion for identifying comparable cases. Unfortunately, normalization is a legal process/event that says relatively little about the actual quality of a relationship. In particular, its significance with respect to war and peace is unclear. Normalization is no guarantee of peace; most belligerent countries have normal relations prior to the outbreak of conflict. Conversely, the severing of official ties need not imply that war is likely. Consider, for example, the U.S. government's withdrawal of recognition from the Republic of China (Taiwan). In fact, normal diplomatic relationships may range from acutely hostile to quite cordial, while those that are not normal may also vary considerably. To his credit, Armstrong acknowledges that normalized relations do not "constitute the substance of improved relations" (p. 6). But what does? While an answer is provided for each specific case, the book never offers a substantive definition of rapprochement as a general phenomenon, as an analytic category. The result is a work in which (1) the meaning of rapprochement (normalization) in practical and theoretical terms is not clearly articulated and (2) the cases being studied are not entirely comparable. As Armstrong's discussion shows, normalized relations between Egypt and Israel were quite different in substance from those between East and West Germany and between China and the United States.

Although it may correlate only weakly with the substance of a diplomatic relationship, normalization does signify some improvement in relations between states that previously refused to recognize one another. If the cases examined in this volume are any indication, such improvement is typically modest, and "breaking the ice" is often the first step in a process that policymakers hope will continue on into the future. For this reason, scholars interested in the subject will need to concern themselves with the *consequences* of normalization as well as its causes. An important question is whether strategies of negotiation conducive to normalization may ultimately be counterproductive from the standpoint of improved relations. Armstrong concludes, for example, that "ambiguous formulations allowing of differing interpretations should be used to overcome disputes over matters of central principle" (p. 134). Papering over disagreements may enhance the prospects for normalization, but the approach seems likely to create difficulties down the road. One explanation for the collapse in the late 1970s of Soviet–American détente (not a case of normalization, of course) is that its founding document, "Basic Principles of Relations between the United States of America and the Union of Soviet Socialist Republics," was too vague. Each side adopted its own interpretations, which were then violated by the other side, leading to mutual recrimination and charges of bad faith. While "ambiguous formulations" may be beneficial to the short-term objective of achieving normalization (or some other limited improvement in relations, e.g., détente), they may be unhelpful, even damaging, to the chances of more fundamental rapprochement over the long term.

Because Armstrong is interested primarily in process, it is not surprising that the book is weaker in its analysis of the structural conditions favorable to rapprochement than its treatment of strategies and tactics. Armstrong hypothesizes that "initiatives to improve relations with an adversary will succeed when . . . the costs of maintaining hostile policies are growing or threatening to grow" and when "the prospects of gain through such policies are significantly diminished in comparison to previous expectations" (p. 23). In principle, the costs and gains associated with hostile policies may be internal as well as external. Nonrealist scholars have long argued the importance of domestic sources of international behavior. From a fundamentally realist perspective, Steven David has proposed that classical balance-of-power theory be amended to account for the fact that alignment decisions of Third World states are frequently based on considerations of internal, rather than external, security (*Choosing Sides*, 1991). Armstrong is aware of the potential significance for rapprochement of domestic conditions; but fearing that to explore both domestic and international circumstances would detract from his inquiry into process, he defines structure in terms of international circumstances alone. As a result, his investigation is incomplete.

Fortunately, the book's defects are more than offset by its virtues. The cases it examines are important and deserve comparative analysis. Armstrong's case studies, based mainly on secondary sources and a few autobiographical works, fulfill their purpose, which is not to provide complete, historical explanations but to test and modify a set of hypotheses developed in the opening chapter. Indeed, this volume is exceptional for the clarity with which the case studies are informed by theory (and vice versa). An empirical investigation into the causes of peace, *Breaking the Ice* breaks new ground. It is worth reading.

Vassar College STEPHEN R. ROCK

The Political Subject of Violence. Edited by David Campbell and Michael Dillon. New York: St. Martin's, 1993. 185p. $59.95.

In conventional international relations theory, politics and political philosophy have often been separated. This divide has been reinforced by the legacy of twentieth-century political realism, which proclaimed separate spheres for politics and ethics. This volume, which includes contributions by political philosophers and critics of conventional international theory, attempts to bridge this gap. Drawing upon Weber, Neitzsche, Heidegger, and many contemporary philosophers influenced by them, the authors center their analysis on the relationship between reason, violence and politics.

All contributors share a pessimistic view of the modern era, which, they claim, "has no ethical discourse capable of responding to its lethal global predicament" (p. 19). Influenced by continental European philosophers deeply troubled by the legacy of two world wars, facism, and the Holocaust, they see a more intimate relationship between modern instrumental reasoning and modern political violence than contemporary strategists writing in a rationalized Clausewitzean tradition.

Over the past three centuries, a Eurocentric world has achieved security within by banishing legitimated violence to the boundaries of the international system, between frontiers of newly emerging states or colonized territories. But in the post–Cold War era, the authors see the potential for violence increasing as traditional boundaries between order and chaos are collapsing in the face of ethnic conflict and technologies of globalized warfare. With the achievement of what he ironically terms "world society," Howard Caygill claims that the distinction between the sovereignty of reason within and violence at the boundary has collapsed. Since our identities are constructed within the constraints of time and space and depend on differentiation from those on the outside, we have no defensible basis for deploying our philosophical categories globally.

The implications of this growing ubiquity of violence and the privileging of identity over difference is made more concrete in Michael Shapiro's essay on the Gulf War as an example of contemporary warfare in which, he claims, the identification of friend and foe no longer depends on political and geographic considerations but on electronic devices that render enemies invisible to soldier and civilian alike. For Shapiro, the implications of this "weapons eye view" are all too obvious. It legitimates large-scale violence by a state whose identity has become intertwined with its arsenal of weapons of mass destruction designed to achieve security vis-a-vis an enemy whose human face has disappeared.

If the unipolar moment, applauded by some recent international relations writings, is not a cause for celebration for these authors, the volume disturbs recent claims of conventional theory in other ways, as well. Drawing on the work of contemporary poststructural French philosophers, Simon Critchley points to the fragility of democracy: when no one holds the place of power, totalitarianism is an ever-present possibility. In an era when posttotalitarian democracies in both East and West Europe are witnessing a revival of xenophobic nationalist thinking and where democracy is constantly being undermined by the violence of ethnic and religious conflict, these authors prompt us to ask whether we can fix our hopes for world order on the claims of an evolution toward "mature anarchy" and the belief that democracies do not fight each other.

This volume also contributes to the contemporary discussion of the meaning of security. Examining the historical evolution of the definition of security in Western political philosophy, James Der Derian offers a Nietzschian interpretation, protection from the unknown, and a desire for certainty as being the most representative of our modern understanding of this term. Yet, claims Der Derian, in our late modern world, not only has the security of boundaries between the known and the unknown disappeared but our attempts to achieve security are no longer adequately grounded in a stable "reality." His analysis of modern warfare points to a "simulation syndrome" where a false confidence in technological imperatives override human intentionality.

Challenging the boundaries within which our conventional understanding of security has been framed is also the subject of R. B. J. Walker's contribution. In an implicit challenge to conventional theories that are beginning to draw boundaries between zones of peace and zones of turmoil, Walker problematizes the traditional levels of analysis of international theory. Drawing on Weber's analysis of the state, he points to the problematic relationship between the violent identities of modern citizens socialized into an exclusionary mode of thinking and the legitimate monopoly of violence by the state, which are mutually constituted in ways that predict the likelihood of violence on the inside, as well as the outside.

Since all of these authors claim that modern reason has been complicit in the politics of violence that, they believe, now threatens our very existence, they assert that we cannot depend on reason to extricate us from these dilemmas, a conclusion that fundamentally challenges the faith in instrumental rationality that lies at the heart of conventional international theory. Is there any way out of this deeply disturbing condition? In the "postface," David Campbell and Michael Dillon suggest "a cultivation of life for no reason" or for reasons of life itself. The ethical implications of this perspective are located in the ideas of Emmanuel Levinas, who sees human beings not as the atomized individuals of Western liberal thought but as essentially social, with identities constituted through their relationship to others. As a prescription for a less violent world, this view of human nature translates into an ethic of responsibility out of which a socially democratic world society, where difference is celebrated rather than denied, could be constructed.

As radical in its conclusions as it is pessimistic about our present condition, this volume fundamentally challenges the core of conventional international theory at many levels. Yet its deeply held skepticism, grounded in a strong sense of alienation from the contemporary world of international politics, is unlikely to convince those who are not already sympathetic. Its complex philosophical arguments make it more accessible to political philosophers than to conventionally trained scholars of international relations. It is a text that will disturb, but it deserves our attention as we struggle with the growing presence of new types of apparently unavoidable violence.

College of the Holy Cross J. ANN TICKNER

Relocating Middle Powers: Australia and Canada in a Changing World Order. By Andrew F. Cooper, Richard A. Higgott, and Kim R. Nossal. Vancouver: University of British Columbia Press, 1993. 232p. $65.00 cloth, $29.95 paper.

One sense in which middle powers have been "relocated" during the 1980s and 1990s, according to Cooper, Higgott, and Nossal, pertains to the structural outcome of processes: changes in their position in the world economy relative to a declining U.S., ascending East Asia, an integrating Western Europe, and disintegrating communism in the Soviet Union and Eastern Europe. A second, more theoretically interesting sense in which the authors use the term *relocating* has more to do with agency: qualitative changes in the kinds of activities and goals that middle powers undertake or, more broadly, in the international role they define for themselves and seek to play. Tacking back and forth effectively between state agency and the mix of constraints and opportunities thrown up by a post-hegemonic, post-Cold War international structure, Cooper, Higgott and Nossal present a persuasive argument that middle powers such as Australia and Canada have been finding and exploiting new opportunities for the exercise of leadership toward the purpose of multilateral cooperation.

A surfeit of leadership from those with the structural basis to supply it—the U.S. and Japan—has created the space for alternative forms of leadership, while at the same time providing middle powers with ample incentives to define such a role and develop the bureaucratic capacity to implement it. Following Oran Young's recent reconceptualization of international leadership (see *International Organization*, 1989 and 1992), the authors focus on non-structural types of leadership, i.e., types not based on leverage derived from preponderant material capabilities. The principal alternatives are intellectual and entrepeneurial leadership: the former refers to formulation of order-building ideas and/or the provision of improved information, and the latter connotes activities such as brokering, mediation or coalition building (though these and other leadership activities, e.g., agenda setting, may straddle the two categories). The theoretical and practical argument set forth in the first chapter is that international leadership is necessarily becoming pluralized in terms of both the forms of it that may be effective in achieving multilateral cooperation and the number and types of countries providing it.

The authors next describe how in the 1980s first Canada and then Australia responded to changes in their international economic environment with administrative reorganization—amalgamations of their foreign and trade ministries designed to facilitate a more economically oriented foreign policy and to provide the bureaucratic infrastructure for the exercise of intellectual and entrepeneurial leadership in multilateral fora. The heart of the book lies in a series of chapter-length case studies of the role of Australian and Canadian diplomacy in recent cooperative endeavors, including the formation of the Cairns Group of agricultural exporters and the Group's role in the Uruguay Round trade negotiations; the genesis and evolution of the Asia Pacific Economic Cooperation (APEC) forum; and coalition building in relation to the Persian Gulf conflict. These are joined by shorter capsule studies of Australian and/or Canadian initiatives concerning arms control, regional security, human rights and the environment.

Though the impact of the Cairns Group had faded by the end of the Uruguay Round negotiations and the APEC process was to gain its own momentum, these are two paradigmatic cases of middle power exercise of intellectual and entrepeneurial leadership on multilateral economic issues. It is difficult to arrive at an alternative interpretation of Australian efforts to build the Cairns Group coalition among fourteen disparate countries, and for its activist role in steering the Group's attempts to shape the Uruguay Round negotiating agenda, to generate confidence building measures and innovative policy reforms, and to mediate and seek compromise solutions between the rigidly-held negotiating positions of the United States and the European Community. Similarly, Australian initiatives to launch the APEC process demonstrate the scope for middle powers to offer innovative solutions in the absence of structural leadership from either the United States or Japan. Indeed, once launched, APEC has since served to catalyze and harness the constructive energies of both of these major powers.

Security issues offer less latitude for leadership initiatives by middle powers, instead putting them in more of a "followership" role and entailing "coalition joining" rather than coalition building. The authors argue that a traditional middle power emphasis on multilateralism accounts for both Canada and Australia initially joining the American-led coalition to diplomatically and economically isolate, and militarily contain, Iraq. But once the United States transformed the purpose of the coalition to the military expulsion of Iraq from Kuwait, the two countries found themselves in a situation, "where the coalition follower, like the queasy roller coaster rider, has little desire to continue the ride but has no option but to hang on tightly until the end" (p. 139). Not all security issues, however, constrain middle powers in this fashion, as illustrated by Canadian and Australian initiatives relating to arms control and nonproliferation and to regional security cooperation. In these issue areas, as well as in issues like human rights and the global environment, both countries have found ways to generate constructive leadership initiatives.

This well organized and clearly written book succeeds in establishing that a focus on the intellectual and entrepeneurial leadership of middle powers yields a significant gain in explanatory power when used to complement more orthodox approaches to the collective action problems inherent to forging cooperative international institutions. The case studies provide an innovative application of Young's plural approach to international leadership and serve to corroborate the excess empirical content claimed for this reformulated concept. Although put to robust use, the expanded leadership concept, qua concept, is not refined or made more precise in the process. But this is a small nit to pick, and not worth pursuing at further length here. For those seeking an antidote to the great power chauvinism and often parochial character of much American scholarship on questions of leadership, hegemony, cooperation and world order, *Relocating Middle Powers* provides an informative and provocative alternative.

University of Nebraska, Lincoln David P. Rapkin

Economic Vulnerability in International Relations: East-West Trade, Investment, and Finance. By Beverly Crawford. New York: Columbia University Press, 1993. 283p. $55.00 cloth, $17.50 paper.

Global economic interdependence renders states vulnerable to external pressures in providing for their national security. After World War II, this concern was most apparent in East–West trade, investment, and financial relations. However, Beverly Crawford argues, the West's Cold War economic vulnerabilities were more apparent than real. Her book asserts that America's failure to assess vulnerability accurately and to recognize available mitigation strategies led to intra-alliance disputes in the West and could imperil policies designed to reduce post–Cold War vulnerabilities.

Ideas of state vulnerability as a limitation to openness are not new. Philosophers from Hume to Hirschman described the potential conflict between a state's pursuit of wealth maximization through free trade and the goal of national security through state control over markets. They cite excess specialization—good for efficiency but potentially harmful to national security—as the latent danger in liberal economic policies. The inability of the state under free-trade conditions to develop or maintain national resources that would contribute to the economic and military power of the state and the possibility of being cut off from essential supplies during an emergency create vulnerability and justify state intervention.

Building on this tradition, Crawford identifies three Cold War vulnerabilities perceived by the West: (1) technology exodus that could contribute to the economic or military potential of the East (particularly the Soviet Union), (2) resource dependence that could leave the West vulnerable to a threatened or actual cut off in the supply of an essential commodity, and (3) debtor leverage that could undermine the Western financial system or force creditors to renegotiate less favorable terms. To cope with these concerns during the Cold War, the United States championed market intervention through export, import, and financial controls.

Crawford maintains that U.S. fears were exaggerated and that U.S. efforts to coerce allied governments to accept America's heightened sense of vulnerability were misplaced. Importantly, she identifies four alternative strategies to state controls that have reduced and will reduce these forms of vulnerabilities: (1) corporate strategies that typically withhold key technologies from importers limit the potential for harmful technology exodus; (2) multilateral coordination (e.g., through COCOM) that achieves normative consensus in meeting shared security goals can effectively reduce national vulnerability; (3) "sacrificing" strategies such as coordination between private and public creditors (including the possibility of debt forgiveness) can reduce vulnerability; and (4) for vulnerabilities associated with post-communist-era governments, the establishment of stable property rights on the basis of market rational behavior can reduce vulnerabilities associated with the breakdown of order (e.g., weapons proliferation and unsound economic policies that threaten creditors' rights).

The book's empirical chapters examine the vulnerability claims of the "economic nationalists"—essentially those in the Reagan administration who advocated extensive government control over trade, technology transfer, and credits to the East. Their claims included the notion that technology transfer narrowed the technology gap between West and East and contributed to significant advances in Soviet military capabilities, that Western Europe was vulnerable to a cut-off of Soviet natural gas shipments, and that Eastern debtors could threaten Western banks and the Western financial system with default. With the exception of mutual financial vulnerability, Crawford finds the evidence insufficient to justify America's heavy-handed attempts to compel its allies to seek greater market control through government action.

The empirical chapters lend credence to her claim that the Reagan administration exaggerated Western vulnerability. The evidence in support of her argument is generally convincing, if at times uneven. For example, in considering whether Western technology transfer contributed substantially to economic growth in the Soviet Union, she relies on studies of Soviet technology absorption made during the 1970s and 1980s. Although these studies cast doubt on the claims of the economic nationalists, who (she rightly notes) did little to justify their claims of vulnerability with contrary evidence, they are limited in scope.

The book could also benefit from addressing other issues in greater depth. *Why* the threat was exaggerated is a critical question left to a brief discussion in the concluding chapter. There the author concedes that the Reagan administration's exaggerated vulnerability claims might be explained by misperception based on ideology or by bureaucratic self-interest. Although the author has placed this question outside the scope of her study, it carries implications for her book's premise, namely, that vulnerability can be assessed more rationally and that accurate assessment and a better understanding of mitigation strategies can lead to better policy. If threat perception is a function of ideology, however, then it may not respond to a more rational framework and better data. Likewise, if policy is determined by bureaucratic self-interest, then outcomes are the result of a political, rather than an analytical, process. Either way, these answers challenge the notion that a better framework for assessing vulnerability would have led to more effective policymaking during the Cold War and can do so in its aftermath. Similarly, the book does not examine *how* the Reagan administration sustained its vulnerability claims in the absence of empirical support and in the face of opposition from its allies and American exporters. The answers to that question also may turn on ideology and institutional strength and question the author's emphasis on rational decision making.

These concerns aside, Crawford's conceptual framework and its application go a long way toward clarifying thinking both as to the sources of state vulnerability and the available mechanisms for ameliorating such dangers. Her insights improve our understanding of alliance dynamics during the Cold War and could conceivably contribute to more effective policymaking in addressing post–Cold War vulnerabilities stemming from trade, technology transfer, and financial flows.

Georgia Institute of Technology WILLIAM J. LONG

Sino-Soviet Normalization and Its International Implications, 1945–1990. By Lowell Dittmer. Seattle: University of Washington Press, 1992. 382p. $35.00.

This book is basically a new and systematic interpretation of Sino-Soviet relations from 1945 to 1990, utilizing an approach the author calls "prismatic." Dittmer has taken a prismatic approach in the sense that he uses a series of analytical frameworks in his analysis of different aspects of the Sino-Soviet relationship at the different stages of development, and, in turn, makes a major contribution to our understanding of Sino-Soviet conflict and cooperation.

Drawing on a wealth of primary and secondary sources in several languages Lowell Dittmer provides a fresh analysis of fundamental changes in the Sino-Soviet relationship and brings the reader up to date on the most recent developments, including the Gorbachev-Deng summit meeting in Beijing in May 1989 and its implications for the great power triangle between China, Russia and the United States. The demise of the Soviet Union and the creation of the fifteen independent republics of the Russian Federation that has succeeded the former Soviet Union has not fundamentally altered the author's argument in the strategic triangle model but its implication is perhaps more significant than the author has assumed at the time of concluding this book in 1990.

Instead of taking a narrative and historical interpretation of Sino-Soviet relations, the author has taken a number of analytical approaches, such as convergence theory, linkage theory, and development/modernization theory in his explanation of cooperation and conflict, frequent transitions from one stage of development to another, patterns of economic development, the search for national identity, and the struggle for international power and security. Dittmer has operationalized these theories in the context of Sino-Soviet relations and succeeded in establishing the linkage between area studies and political science, comparative politics and international relations, domestic politics and foreign relations, as well as the synthesis of the Eastern and Western modes of thought.

The developmental perspective Dittmer uses in Part I focuses on the general theory of convergence, which assumes that industrialization and modernization imply convergence and that convergence implies rapprochement in Sino-Soviet relations. He argues for the connection between domestic development and bilateral relations between China and the former Soviet Union. Part II looks at China's search for national identity in terms of China's relationship to meaningful international "reference groups." Part III focuses on the security dimensions of Sino-Soviet relations and develops the Strategic Triangle Model, which might be the best and most sophisticated model developed for the study of the Sino-Soviet-American relationship and for international relations generally. Dittmer develops the model on the basis of rich empirical and historical data presented in chapters nine through fifteen. Dittmer's data set for the model, however, could have been more extensive. In the post-Cold War era, the archival materials of the past are now made available to scholars in Beijing, Moscow, and Washington. Dittmer therefore could have utilized some of these documents to test his model further, instead of relying on the secondary sources that were utilized, even though the Gorbachev-Deng summit in 1989 marked the "end" of the triangle.

This book is highly theoretical in its analysis of Sino-Soviet relations and may be used to predict the future development of rapprochement between China and Russia. Dittmer makes a significant contribution to some of the core concerns of comparative communism and political development by integrating them into the theory of international conflict management.

University of Connecticut, Storrs ILPYONG J. KIM

Political Theory, International Relations, and the Ethics of Intervention. Edited by Ian Forbes and Mark Hoffman. New York: St. Martin's, 1993. 249p. $69.95.

This volume grapples with an old problem, but does much more than rehearse familiar debates between realists and liberals, or between cosmopolitans and communitarians. It does much more than address the standard questions of whether (or when) intervention in the affairs of other states may be justified: it forces us to consider whether the very concepts with which we often begin to answer these questions—the state, sovereignty, legitimacy, law, prudence, right, obligation—are the appropriate starting points for ethical inquiry. In so doing, it brings to bear a wide variety of competing approaches and perspectives, and it makes great effort to combine theory and history. It therefore breaks all of the rules of academic compartmentalization. Its iconoclasm and eclecticism are quite refreshing, and the result is an extremely thought-provoking book.

Following an introduction and overview by the editors, the book is divided into four parts of three to five essays each. Part I, "Definitions and Dilemmas," begins with a solid literature review by Richard Little, and offers preliminary cuts at the problem of intervention from the perspective of international law (Anthony Carty), the history of Western political thought (Cornelia Navari), and analytic philosophy (Peter Johnson). Part II, "Theoretical Perspectives," offers a critique of Realism's approach to the problem of intervention (Leo McCarthy), a pragmatic defense of the principle of non-intervention (Caroline Thomas), a rights-based justification of intervention grounded in a doctrine of basic human needs (Raymond Plant), and an Aristotelian analysis grounded in an account of virtue (Barrie Paskins). It ends with an essay by R.J. Vincent and Peter Wilson that is generally critical of these approaches, suggesting that the problem of intervention will remain intractable until international relations theory has access to an appropriate conception of international legitimacy. Part III, "Hard Cases," includes essays on Israel's intervention in Lebanon (Ali Sadeghi), Vietnam's intervention in Kampuchea (Michael Leifer), international sanctions against South Africa (John Hoffman), and the plight of the Kurds in northern Iraq (James Mayall). Part IV, "Theoretical Departures," seeks guidance on the ethical problem of intervention in contextualism (N. J. Rengger), critical theory (Mark Hoffman), and Foucault (Ian Forbes).

Many of these essays are gems. Anthony Carty, for example, makes brilliant use of the decision of the International Court of Justice in *Nicaragua* v. *US* to demonstrate the weaknesses of international law as a basis for ethical judgment—weaknesses inherent in the paradoxical idea of an authoritative body of law grounded in the actual practices of states, and in the tendency of international lawyers and justices to confuse regimes with the states they govern, begging many of the most important ethical questions. Similarly, Leo McCarthy deftly unveils the paradoxes and inconsistencies of Realism's treatment of the problem of intervention, concluding that the moral egoism upon which Realism is founded means that "[t]he state has available to it no arguments to prohibit intervention against itself, and so must deny the moral significance of its own predication" (p. 87). But while these and many of the other essays in Parts I through III do a superb job of explaining why none of the most common principled positions on the problem of intervention is entirely satisfactory, they do not point us in the direction of a solution. By the time the reader reaches page 179, he or she begins to despair of finding any moral compass at all.

It is this despair that Part IV is intended to address. And the essay with which it opens, N. J. Rengger's "Contextuality, Interdependence and the Ethics of Intervention," is the highlight of the volume. Rengger masterfully surveys the weaknesses of traditional approaches to the problem of intervention, and argues (correctly, in my view) that the international system has evolved in such a way as to render them anachronistic. The international system may still be state-centric, but states are far weaker vis-à-vis non-state actors than ever before, and are pulled from without and within by the simultaneous and contradictory pressures of globalization and fragmentation. The result is a greatly complicated moral landscape no longer susceptible to foundationalist analysis. He concludes by making an appeal for a contextual approach to ethical judgment, and teasingly suggests (also correctly, in my view, although perhaps too quickly to be persuasive to the skeptic) that contextualism is neither relativism nor an open invitation to arbitrary judgment.

With the reader eager to hear more of what Rengger has in mind, the volume abruptly shifts gears with the final two chapters, in which the editors offer their own visions of the answers to the problem of intervention. I found these chapters frustrating for many of the same reasons that I find critical theory and deconstruction frustrating in general. Both are motivated by entirely laudable goals: addressing the unselfconsciousness of many scholars about their key concepts and axioms; correcting the illusion of pure objectivity in the study of human affairs; making the link between theory and practice more obvious; baring the dominance—and thereby the historical contingency—of particular meanings, practices, and relationships; cataloguing their effects; and opening our minds to how the world might be different. But while these are entirely laudable goals, they do *not* require radical epistemological departures; they do not justify relaxing our rules of inference or the precision of our concepts; and they do not entirely blur the distinction between studying the world and changing it. They are goals that careful scholars can pursue within the rubrics of empirical social science and analytic philosophy, whose comparative advantages in clarity and rigor are fully compatible with creativity and circumspection. Hoffman's pitch for critical theory, and Forbes's pitch for deconstruction, represent calls to abandon ship. But they raise a number of red flags that should make us wary of so doing. Forbe's chapter is built upon the foundation of a concept—power—that is problematic enough when used restrictively by mainstream social science, but that is stretched beyond all usefulness by Foucault; Hoffman's openly embraces a political agenda that is not only self-contradictory but prejudicial to the very ethical questions that it is ostensibly seeking to answer ("A long-term goal would be the promotion of forms of radical democratic political communities in which all inhabitants would have an equal right and opportunity to participate in a way that would not entail any enforced denial of identity" [p. 205]). Both chapters raise serious questions about the radical subjectivity of their interpretations, and what Hilary

Putnam would call the "rational acceptability" of their propositions.

Turning from individual essays to the volume as a whole, the editors should be complimented for the extent to which many of the essays manage to speak to each other directly and engage each other in debate. Clearly they have made a considerable effort to put together a volume that coheres. In this they are largely, if not entirely, successful. Caroline Thomas's prudential defense of the principle of non-intervention, for example, immediately follows Leo McCarthy's brilliant exposé of the logical contradictions inherent in that position, and exhibits all of them. Yet it makes no attempt at rebuttal. If the two had been reversed, at least they might have been seen as point and counterpoint.

More seriously, although a number of essays note that the concept of intervention can refer to a wide variety of acts and relationships (and, in its most liberal interpretation, is coextensive with international politics as a whole), no two essays seem to use the word in precisely the same way. It is therefore often difficult to know exactly what problem the book is seeking to explore. This indeterminacy raises particular difficulties for the authors of the case studies, all of which are interesting and informative, but none of which is truly a "hard case" in the absence of a clear definition of the phenomenon. It is possible, of course, that the editors felt they could not impose a single conception of intervention upon their authors without fixing analytical parameters of the kind that they regard as problematic; for doing so would seem to require taking "states" or "cultures" or "communities" as basic units of analysis. But one can address the philosophical questions the volume raises in the context of a more tightly-focussed discussion of a particular class of acts, and some readers will wish that the editors had worried less about form prejudicing substance and more about the tractability of the book as a whole.

Still, the book very effectively challenges the reader to think about the problem of intervention in new ways and to grapple with the profound issues that it raises, and anyone seriously interested in the ethical problems of intervention will want to read it.

University of Toronto DAVID A. WELCH

Human Rights and Peace: International and National Dimensions. By David P. Forsythe. Lincoln: University of Nebraska Press, 1993. 206p. $35.00 cloth, $13.95 paper.

To date, the debate about democracy and peace has had little fall-out in the human rights literature. This is surprising, because human rights and democracy are so closely related. David P. Forsythe's book, the first in a new series on human rights edited by himself, sets out to fill this gap.

Forsythe concentrates primarily on the first generation of human rights, namely, civil and political rights, and secondarily on the second generation, namely, social, economic, and cultural rights. He gives short shrift to any notion of a third generation of rights to peace, development, common heritage, and a healthy environment, discounting UN resolutions on these topics as mere diplomatic rhetoric. The right to peace has never been formalized into a legal principle; more important, it has no independent and specific meaning beyond already familiar concepts in international law such as *jus ad bellum*. Proclaiming the right to peace does not tell us whether we should embrace unilateral disarmament, peace by nuclear deterrence, or preemptive disarming of potential aggressors. In making this stand, Forsythe circumvents the easier solution of creating a *definitional* link between human rights and peace and sets himself the more ambitious task of investigating the *empirical* link.

While Forsythe strikes a middle ground in relation to human rights—retaining second-generation but not third-generation rights—he adopts a narrow concept of peace as the absence of direct physical violence. He examines the general relationship between peace and human rights at the level of interstate war, then in relation to covert action, and then in relation to domestic war. Nearly half the book is devoted to three case studies of domestic war—in Sri Lanka, Liberia, and Romania.

With regard to interstate war, Forsythe joins the broad consensus that established democracies do not fight each other. He finds realism, still dominant in U.S. international relations textbooks, to be deficient in not paying much attention to human rights and an emerging international morality but still worthwhile for dealing with interaction with authoritarian states. Democratic countries are fully capable of acting contrary to international law, committing aggression, and violating human rights in armed conflict, although probably less so than authoritarian states. Even in dealing with newly emerging participatory polities, democracies may resort to force, usually covertly, as the United States has done in Iran (1953), Guatemala (1954), and elsewhere. While the opponents frequently were not fully democratic, what led the United States to intervene was primarily the combination of economic concerns and perceived national security, rather than concern for democracy. National security concerns transcend the Cold War and are based on an informal American ideology of greatness, racism, and hostility to revolution. David Forsythe leaves open the question whether "covert war" against radical quasi-democracies is a specifically American trait or whether other democracies are merely content to let the United States do their dirty work for them.

Reverting to his discussion of overt international war, we should note Forsythe's view that avoidance of war is not fully accounted for by political democracy. He embraces what he calls the "developmentalism" of John Mueller in *Retreat from Doomsday* (1989), who argues that not only democracies but also developed states in general have long sought to avoid war, because it is costly, as well as morally repulsive. Giving this argument an original twist, Forsythe links Mueller's ideas about war avoidance to the second generation of human rights, thus broadening the human rights–peace nexus. While this is an interesting argument, it suffers from the lack of any systematic empirical evidence of any gain in explanatory power by adding the second generation of rights. Besides, the increasing openness at the end of the Cold War has raised serious questions about the fulfilment of second-generation rights in countries that deny their inhabitants political freedom, as distinct from the window dressing at which many such countries excel. Thus, whether it was developmentalism, deterrence, or other factors that led the Soviet Union to pursue a relatively moderate foreign policy must remain unanswered. Finally, Forsythe is less original than it might seem in shifting the attention from democracy–peace to human rights–peace. Rudolph Rummel, in his early work on the subject, spoke not of democracy but of "libertarianism," emphasizing freedom (including political rights), rather than democracy in the sense of competitive politics and majority rule; only recently did he adjust to the prevailing terminology.

Forsythe's views on human rights and domestic war are less precise. Interpreting revolution as a crisis in legitimacy, he outlines six sources of domestic legitimacy (legal, moral, historical, ideological, personal, and functional) and three sources of international legitimacy (legal, moral, and political) and sees the nonfulfilment of human rights as figuring prominently in some but not all legitimacy crises. He suggests that it may be possible in the future to move towards a predictive theory, but for the time being he is satisfied to remain at the level of a "typology of preconditions" and to call for an analysis of case studies in order to discern which sources of legitimacy are invoked. At this level of precision, we cannot expect too much of the case studies. They are quite interesting, but since they have been selected precisely as cases of domestic war, nothing can be said about the conditions under which different legitimacy deficiencies may coexist with more benign outcomes than war.

Forsythe's book is of interest as a pioneer effort toward forging an empirical link between human rights and peace and specifically because of his views on the peace-building effects of the social and economic rights. That case cannot be regarded as proven but will hopefully inspire systematic empirical study in the future.

International Peace Research Institute, Oslo NILS PETTER GLEDITSCH

Business and Banking: Political Change and Economic Integration in Western Europe. By Paulette Kurzer. Ithaca: Cornell University Press, 1993. 261p. $39.95.

Paulette Kurzer has written a book that will make a substantial contribution to our understanding of national adaptations in the 1980s to changes in the international economy. The institutions and political arrangements of small democracies in Europe are widely thought "to be adept at coping with the repercussions of international crises and changes" (p. 13). Cooperation across class divisions, social partnerships, are usually considered central to this capacity for adaptation. The partnerships, it is increasingly clearly understood, must serve the interests of both labor and business. From the standpoint of labor, the "political trade-offs" depend on the expectation of workers that current sacrifices yield higher earnings and more jobs later" (p. 9). The business community, work by Jonus Pontusson and Peter Swenson has shown, gains from the predictability of labor relations as long as national wage rates are set by bargains in the internationally exposed export sectors. But, Kurzer observes, these partnerships "have not fared well during recent periods of dramatic international changes" (p. 15). Implicitly, these are the countries most likely to be able to adjust to the new global economy. The capacity of Social Democracy to provide a distinct political strategy for economic troubles is at issue. Moreover, if these countries are unable to maintain their core political bargains that permit adaptation, then other countries with even weaker capacities for adaptation will find it difficult to avoid changes forced by globalization.

Using the cases of Austria, Belgium, the Netherlands, and Sweden, Kurzer sets out to explain why the cross-class social partnerships have broken down. The argument is that globalization with capital mobility and financial integration breaks down the basis of these compromises. It both augments the power of business and diminishes business interests in the bargains that underpin the partnerships. Kurzer effectively constructs the analytic foundations of the argument building on models of Prezworski and Wallerstein. Notably, she highlights the growth of transnationalism and its effect on social protection and compensation rather than an exhaustion in labor power resources. Governments are driven into a competition for the favor of business. To curry favor they seek to adapt their institutions to the preferences of business and finance. She next contends that the national response varies with the character of the ties of business and finance to the international economy. "Small countries differ in the way in which they are linked to the global economy and international financial markets, and this difference defines the extent to which capital can move from one market into another or from one form of investment into another" (p. 16). She defines a country's external relations by the international and export activities of business and finance. The result of these differences is that business and financial interests will vary, state policy will vary, and central bank policy will vary. The basic bargains are under pressure or have broken down in all four cases. Those most exposed—Belgium and the Netherlands—have seen the deepest break. Those with a more muted international orientation—Austria and Sweden—have "not escaped unharmed," but they have been able to at least dampen or retard the consequences of globalization. Kurzer's argument is effectively presented and the evidence in the form of these four cases is convincingly mobilized. She concludes pessimistically that "because the trend in the advanced industrialized world is toward ever-increasing financial integration and business internationalization, national accords between labor, government, and capital are unlikely to reemerge."

The reader is forced to think through the nature of changes in the international economy and their influence on domestic politics. Kurzer is persuasive, but is she right? The effective presentation of the argument and evidence obscures a number of very fundamental questions that must be debated before we draw conclusions. Let us consider each of the elements of the argument in turn. First, globalization, like interdependence, is a catchall phrase that does not specify relationships. To Ford, globalization meant sourcing parts from the globe for a car designed for all markets. For Toyota it has meant building up entire production systems within each global region. Consequently the precise nature of developments must be clarified. Consider finance. The numbers demonstrate the vast increase in international financial markets. Yet those global markets are largely wholesale markets, and in fact may consist heavily of trade in derivatives that act as insurance against a more limited body of underlying transactions. In any case the transformation of savings into investment remains for now largely a national matter. Do national financial markets endure despite liberalization, regulation and reregulation, securitization, and internationalization?

Arguably they do. Certainly there has been change, but it remains an open question also whether these national markets have converged or evolved along national trajectories. Consider national technology systems, which are increasingly understood to be national and to be at the heart of economic growth. Many international alliances often look like a trade of technology by companies rooted on different technology trajectories. Indeed, multinational firms may roam the globe but with a very few arguable exceptions, they have national homes and indeed their roaming is more focused on their home region of the globe. IBM's intense effort to reinforce the American electronics supply base by pushing for national policies emphasizes that corporate choices may be more limited than suggested at first glance. Particular to this book, one must observe that the greater economic ties in Europe do not amount to a broad drive to globalization. Rather they are creating a more tightly woven regional economy. Regionalization and globalization are not the same thing. And indeed if this is a story of Europeanization, where economic integration has been driven by political choice, the character of causation is much more complicated than suggested here.

If globalization is an ambiguous concept, the interests of business and the influence of the international economy on those interests are even fuzzier. Second, it is likely that the very terms of market competition, not the extent of "globalization" are forcing the breakdown of national social bargains. For example, business interests have become more heterogeneous. The heterogeneity makes it more difficult to strike a single national wage and work bargain. New mechanisms of bargaining have to be established. The emergence of organized white collar labor in significant sectors sheltered from the competitive international marketplace has also undermined national wage bargaining by breaking the link between productivity and rates. Competitive pressures for increasing the speed of product to market and the introduction of new production systems requires a rethinking of how to balance worker protections. Arguably neither cost of social welfare nor comparative wages, but the influence of the operation of the welfare systems on corporate strategy has driven business to distance itself from particular historical bargains. The Swedish tax reforms for example affected firm-level incentive structure, not total national revenue. Similarly, and ironically, in the United States and elsewhere current demands for worker participation and involvement come from market pressures and from businesses that find that the new production and development systems require higher levels of worker commitment and skill. Again specific to this book, I am convinced that the existing political arrangements in these countries have broken down and that changes in the international setting and economy are contributing to that. I am less persuaded that the particular case is the whole story or even necessarily the core of the story.

In sum, Kurzer's tight presentation makes her case well. At the same time it does not confront the complexity of the changes in the emerging international economy. We do not need to conclude so rapidly that one particular development in the international economy defines a broad and complex evolution or that one specific argument has such dramatically greater power than all others that we can dispense with analytic diversity. More generally, the broad application in political science of the econometrics metaphor of line-fitting to existing lines of argument has in fact slowed our ability to develop new concepts to account for complex phenomena. As a result we may rush too quickly to conclude that something called globalization is afoot and that it will drive national convergence and shift the relative power of social groups. On balance this is a valuable book and a fine achievement. But like many other works that try to assess the domestic political implications of a changing international economy, it still seems uncomfortably constrained by older and still poorly defined concepts that are ready to be replaced.

University of California, Berkeley JOHN ZYSMAN

Conceptualizing Global History. Edited by Bruce Mazlish and Ralph Buultjens. Boulder: Westview, 1993. 253p. $59.95 cloth, $15.85 paper.

Past as Prelude: History in the Making of a New World Order. Edited by Meredith Woo-Cummings and Michael Loriaux. Boulder: Westview, 1993. 254p. $55.00 cloth, $16.95 paper.

These two books are a significant addition to the growing body of literature about globalization and global trends—from the earlier literature about the global economy (perhaps going back to *Wealth of Nations* 1776) to the more recent concerns about the global city (King 1990).

Conceptualizing Global History is breaking new theoretical ground and fine-tuning old concepts. Contrary to the widespread belief that the end of the Cold War and the collapse of communist power in Europe signify the discrediting of globalism, Mazlish is convinced that those developments have removed impediments to the true forces of globalization which were for sometime shackled by the artificial ideological frontiers of both communist and capitalist demarcation. The old divisions between the First World (of advanced capitalism), the Second World (of advanced communist countries) and the Third World (of developing societies) is giving way to a neater global dichotomy between North and South, which is itself in the process of mutation as newly industrializing powers exert their influence on the world economy. Raymond Grew warns that we may need special methodologies to deal with the complexities of "modernization" (pp. 229–230).

But here the Mazlish and Buultjens volume warns us against equating global history with world history. One variety of world history is the multicultural school which seeks to restore respectability to all cultures and transcend the persistent bias of Eurocentrism. Examples cited in the Mazlish and Buultjens book include William McNeil and Philip Curtin.

Another kind of world history is "Braudelian," associated with Fernand Braudel and the paradigm of "world systems" of Braudel's distinguished disciple, Immanuel Wallerstein. The Braudelian approach emphasizes material factors, underplays the importance of ideas, and until recently had a more restricted time frame.

Wolf Schäfer, in his chapter "Global History: Historiographical Feasibility and Environmental Reality," also accuses *world history* of trying to be "total history" (p. 51). The agenda is the whole span of time and the whole geography of the human race.

Global history, according to Mazlish and Buultjens, also has to be distinguished from "universal history"—"the effort to encompass all of history, not just the recent past, in one sweeping account" (pp. 3, 97). Perhaps Marxian historical materialism with its epochs of ancient, Asiatic, feudal, capitalist, socialist, and communist modes of production may be regarded as one school of "universal history."

If global history is neither world history nor universal history, what is it? It is not merely a globalist approach to the study of different local histories. The global historian studies the actual processes of globalization in the real world—from the spread of technology to the internationalization of the ethics of human rights. Manfred Kossok, in his piece "From Universal History to Global History," interprets universal history as involving hegemony (pp. 106–107) while global history recognizes "manifold forms of humanity."

"But who is the poet-laureate of the global village?," Richard J. Barnett and John Cavanagh ask in their own chapter on "A Globalizing Economy" (p. 153). There were bards and court musicians for the rise of the nation-state. Do we now look to corporate managers, environmentalists, business planners, advertisers, television producers, or e-mail enthusiasts as the new celebrants of the global village? The authors of these essays seem to underestimate the extent to which the main agent of globalization may be the nation-state itself—in the process of "digging its own grave." Funding for globalizing technology, tariffs for globalizing trade, and political pressures in pursuit of globalizing human rights (p. 175) all include a disproportionate role by the nation-state itself.

Ralph Buultjens poses tantalizing questions about "Global History and the Third World." He concludes that until recently most parts of the Third World did not study societies other than their own and in any case they had different concepts of *time* in their cultures (p. 75). Were societies of high social consensus in Africa and Asia inclined to be low in cross-cultural curiosity? Post-feudal Europe placed less of a premium on internal consensus, developed a high curiosity about other societies, and set out to discover, explore and conquer the rest of the world. Ralph Buultjens' analysis invites us to conclude (inspite of himself) that global history, for the time being, has to be Eurocentric.

This book is fascinating not only in terms of what it says itself but also in terms of where it places itself in the wider debates of the epoch. How does global history relate to postmodernism? Bruce Mazlish takes us through the concepts of time, progress, universalism, materialism and David Harvey's phrase "the beginning of the global day" (p. 123). Mazlish also reminds us how the computer and the communications revolution may be fulfilling Jacques Attali's prediction of the rise of "the wealthiest nomads . . . freed to get through the world, permanently attached only to their friendships and their work" (p. 123). Is Salman Rushdie (imprisoned as he is by an Ayatollah's *fatwa*) such a nomad? Rushdie sees himself merely as a migrant "perhaps the central or defining figure of the twentieth century" (p. 123).

Wang Gungwu's chapter on "Migration and Its Enemies" is suggestive on several fronts (pp. 131–151). But the chapter seems to ignore situations where the enemies of migration are, in the first instance, the migrants themselves. This, of course, includes the trans-Atlantic African slave trade—a large-scale case of involuntary migration. Wang Gungwu is sensitized to the Jewish Diaspora (p. 143) but not to the African Diaspora.

There are three main gaps in this book: the globalizing impact of the *arts* (herein represented only by music), the relevance of globalizing *sports* and, most surprising of all, the globalizing implications of *war* and preparation for war. John Joyce's essay on "The Globalization of Music" (pp. 205–224) does go some way towards filling the artistic gap. But in this sense the arts include both popular and high art—and range from Shakespeare to soap-operas, from printed media to television and the cinema. They have all had globalizing implications on the whole. They are missing from this book.

In his introduction Bruce Mazlish proclaims "Ours, to put it categorically, is an age of Globalization" (p. 1)—but he then promptly forgets "the century of global *wars*." Arming for planetary destructiveness has been a unique peculiarity of this century—from the assassination of an Archduke in the Austro-Hungarian Empire in 1914 to the formation of the North Atlantic Treaty Organization (NATO) in 1948. War as a globalizing experience is not part of this book. It is indeed a major gap in an otherwise impressive agenda.

A more modest ambition underlies the book edited by Meredith Woo-Cumings and Michael Loriaux, *Past as Prelude: History in the Making of a New World Order*. The historical period covered in this volume is more clearly within our own twentieth century, with special reference to what Bruce Cummings' Chapter 2 describes as the "seventy-years' crisis" (1919–1989). A European civil war which broke out in 1914 was in the process of being settled in the 1990s.

This book was evidently completed before the worst news broke about Bosnia, or about the uncertainties concerning Russia, or about the political tensions of most of the former republics of the Soviet Union. While Germany has indeed been re-united, Czechoslovakia, the Soviet Union and Yugoslavia have broken up. This book has the sense of optimism of three years ago that "the league of the North" was not at peace—and that conflict and rivalry has been relegated to the Southern hemisphere (p. 5). Unfortunately, a final European settlement to close the chapter of the First World War seems as elusive as ever.

Has Germany been irreversibly "contained" through the triple mechanisms of domestic democratization, regional economic union, and involvement in the NATO? Peter Katzenstein is satisfied that the end of the Cold War and German reunification will strengthen rather than weaken internal democracy and regional good-neighbor policies (pp. 78–81). Michael Loriaux is more circumspect about Franco-German relations and the long-term stability of Western Europe. He asks, "Is it so inconceivable that a demagogue might arise in the heat of electoral competition in France—or even in Germany—and proclaim with success that the French or Germans were getting nothing out of the European Community but unemployment and runaway immigration" (p. 109)? The "Community" has since been re-named the European Union.

The successful "containment" of Japan is also explored in this volume, but the full implications of the end of the Cold War for Japan are less deeply investigated than they are for Western Europe. Moreover, Meredith Woo-Cummings' concept of "East Asia" is too narrow and too Americocentric to deal with the spectacular economic growth rates in the People's Republic of

China in the years when the USSR was disintegrating. China was also on its way towards becoming the largest steel producer in the world. Basically this book marginalizes China and the Middle East, ignores India and South Asia, and forgets the existence of Africa.

The Third World should be grateful that at least Latin America gets stimulating treatment under the pen of John H. Coatsworth. And do we see Russia's future as a possible Third World economy hang in the balance in Valerie Bunce's comparison of the reforms of Alexander II and those of Mikhail Gorbachev?

While authors like Franz Schurmann bring fresh insights about the Gulf War, and while we do explore afresh in these pages the implications of the end of the Cold War, this book is less about the New World Order and more about the *new trilateralism*—the United States, Europe (now both East and West), and Japan and her sphere of influence. Is this kind of trilateralism simply a more sophisticated version of Eurocentrism? The world is a larger place than this book implies.

SUNY, Binghamton Ali A. Mazrui

The Internationalization of Communal Strife. Edited by Manus I. Midlarsky. New York: Routledge, 1992. 306p. $64.95.

Intercommunal violence despoils many contemporary societies. Newly independent states may be especially vulnerable. Ethnic violence disturbs numerous former colonial territories of Africa, Asia, and the Middle East. Long-established polities are not necessarily immune, however, as witnessed by internal frictions that led to the disintegration of the Soviet Union and Yugoslavia and to ensuing troubles within and among their successor states. Communal strife may pose international, as well as domestic, problems. Its roots are ordinarily local, including competing claims to power or independence based upon asserted differences of ethnicity, nationality, religion, language, or social class. Its consequences, *in extremis*, may lead to major international war. Domestic conflict may acquire international significance by following any of several paths. Dissidents may gain support from foreign governments or from foreign opposition groups. Embattled regimes may obtain support from foreign governments or private sources. Parties to domestic conflict may carry the fight abroad, even in retaliation for aid given their opponents, in support of foreign groups that aid them, or to divert domestic or international attention from internal strife. States not original parties to a conflict may subsequently intervene in order to aid one side or another or to impose peace upon all. Once one foreign entity intervenes, others may follow in aid or opposition to the first. Generally speaking, the more "internationalized" the conflict, the greater the danger to world peace.

The body of recent literature is dispersed and unduly specialized. Numerous monographs describe individual communal conflicts, past and present. Streams of journal articles argue about the correlation between civil and international conflict and the international proclivities of specific types of regimes. Systematic theory and research about how communal conflict is generally internationalized or contained is in short supply, however. Many of the best-known treatments of the subject were penned many years ago, including Eckstein's *Internal War* (1964), Rosenau's *International Aspects of Civil Strife* (1966), Moore's *Law and Civil War in the Modern World* (1974), and Suhrke and Nobel's *Ethnic Conflict in International Relations* (1977). The present volume intends to help remedy recent neglect.

The 13 original essays emanate from a 1991 conference at Rutgers University. Attention is devoted primarily to conflicts since World War II. Several chapters constitute case studies of particular communal conflicts. Recent instances examined in some detail include Argentina (Levy and Vakili), Iraq (Davis), Lebanon (Rasler), Peru (McClintock), South Africa (Ottaway), and Sri Lanka (Johnson). Midlarsky also examines communal strife leading to World War I. Other essays take a larger view. Gurr discusses 88 communal groups involved in protracted or intense conflict since World War II. This is part of a larger survey of 230 communal groups subsequently described in his *Minorities at Risk* (1993). I. W. Zartman discusses incentives and opportunities for international involvement within communal strife based in part upon his previous *Ripe for Resolution* (1989), which surveys African conflicts. Vasquez, previewing part of his *War Puzzle* (1993), examines processes that may lead generally to contagion and diffusion of international violence. Hill and Rothchild explore the relationship between the diffusion of conflict and different types of domestic regimes. Touval expands upon his previously published work regarding foreign mediation. C. R. Mitchell, who has also written before about international mediation, addresses conditions necessary for successful external efforts to promote internal peace.

The collection is partly discordant; it does not pretend to a offer an integrated theory. Each essay adopts distinctive premises and asks different questions. Authors evidently do not agree upon the meanings of *internationalization* or *communal strife*. Formal definitions of these key terms are not provided. The volume lacks a conclusion and a summary bibliography.

The compendium, nonetheless, is more valuable than a mere sum of its parts. It comprises a more representative and more complete survey of recent thoughts about international aspects of communal conflict than is elsewhere available in print. It brings together under one cover elements of wisdom that are meaningful to one another but which are otherwise dispersed and possibly forgotten because buried within singular case studies or associated primarily with specific narrowly defined lines of research. Chapters are grouped loosely according to relevant aspects and approaches, including (1) worldwide perspectives, (2) perspectives of particular states, (3) processes of internationalization, (4) instances of conflict that did not become internationalized, and (5) processes of international mediation. An index helps to guide the reader among chapters. References within each chapter highlight various avenues of research.

One may hope that *Internationalization of Communal Strife* will stimulate renewed effort to develop a general theory regarding the international aspects of domestic strife. It is a politically significant and intellectually demanding subject. The volume offers valuable preparation to those able and willing to embark upon such a worthwhile enterprise.

University of Missouri,
Columbia Herbert K. Tillema

A Normative Approach to War: Peace, War, and Justice in Hugo Grotius. Edited by Onuma Yasuaki. New York: Oxford University Press, 1993. 421p. $69.00.

Once the honorific "father of international law," Hugo Grotius remains a figure of considerable interest—not only to legal scholars but to students of international relations and the history of political thought. Grotius published the first edition of his great treatise, *De jure belli ac pacis libri tres* (hereafter *JBP*) in 1625, during the last and greatest of Europe's religious wars. His aim was to identify all legal rules (laws are rules of moral action imposing obligation) that might limit both the occasions for, and the effects of, organized violence, whether public or private, just or self-serving, and present those rules in "a comprehensive and systematic manner" (*JBP* proleg. 1, 1.9.1). If his ambitions transcended the particulars of sectarian conflict, so, too, did the Thirty Years War and the peace that followed. Grotius inevitably came to be identified with the advent of the Westphalian system of international relations and a secular, rationalist worldview.

Was Grotius an early modern thinker? His stature in Western thought for the century-and-a-half after his death would seem to suggest as much. So do frequent contemporary allusions to a "Grotian tradition." Many of those who have examined his work are less sure. The more carefully scholars read *JBP*, the more cautious become their claims on the author's behalf.

A Normative Approach to War is the most careful, sustained reading of *JBP* in English. Its authors are six Japanese scholars, all members of a research group that began studying Grotius in 1976. A Japanese version of the book appeared in 1987. The English version is updated and abridged; the translation, by Sakamoto Mikio with assistance from James Crawford, is clear and as graceful as the subject permits. The book's authors follow the complete text of *JBP* as Grotius presented it, for convenience dividing the material into nine chapters bracketed by the editor's introduction and conclusion. Clearly a team effort, the book exhibits an exceptionally coherent perspective.

The team's members see Grotius as less modern than the many scholars writing in English who find Grotius a right theorist (pp. 36, 51): "Unlike Hobbes, Locke and Rousseau, Grotius constructed his theory of law without seeking to dismantle the household and the various intermediate corporate bodies. He adhered to the traditional view of man (as *zoon politikon*) that had prevailed in Europe since Aristotle, assuming the existence of multi-layered independent powers. His concept of natural law tended to justify existing political institutions rather than criticize them from the point of view of justice" (p. 9). Although Grotius took issue with Aristotle's conception of justice and distanced himself from the Scholastics, he was uncritical of the Aristotelian tradition in general (pp. 20–22, 26). This interpretation finds support in an extensively cited literature from continental Europe and Japan.

In one respect, Grotius seems modern—not early modern but fully modern. Despite some principled equivocation (*JBP* proleg. 16, 57), "considerations of utility and practicality underlie the whole structure of *JBP*" (p. 347, cf. p. 72). As the Japanese team shows, Grotius' pragmatism stems from his desire to limit the horrors of war by whatever means possible—law in every conceivable manifestation, appeals to conscience, authority and tradition, considerations of prudence. Grotius made his system as comprehensive as he could, indiscriminately adding categories, spinning distinctions, and invoking authorities. By the Cartesian standards of the early modern era, he also made his system a shambles.

Viewing Grotius as practical but not very modern, the Japanese team translates key terms from Grotius' Latin with due caution. They find that Grotius seems to have construed *summum imperium* "as a matter of relative priority in a hierarchy of bearers of *imperium* on various levels" (p. 137). Their Grotius is no Bodin. Thus, unlike the standard English translation of *JBP*, they refrain from using Bodin's term, "sovereignty," except to show that Grotius' resistance to "popular sovereignty" makes him even less an Althusius (p. 138). By translating the term *civitas* as "state," the team does yield to anachronism, despite a sensible discussion of Grotius' corporate conception of "civil society," a term he used interchangeably with *civitas* (pp. 125–27).

For many scholars, the proof of Grotius' modernity is his conviction that there would be a law of nature by which to govern human affairs "even if we should concede that which cannot be conceded without the utmost wickedness, that there is no God" (*JBP*, proleg. 11; cf. Tanaka's article, pp. 26–29, 74–77). If, by this remark, Grotius began the process of secularizing natural law, which Hobbes and Pufendorf soon completed, this was hardly his intent. Rather, it was to support a proposition with an ancient pedigree: human beings have "an impelling desire for society," what "the Stoics called, 'sociableness'" (*JBP*, proleg. 6). Sociableness necessitates society, and this makes human nature "the mother of the law of nature." From this position (which *is* radical in its implications), Grotius could conclude that all law, including law that human beings voluntarily create for themselves, "derives its force from the law of nature" (proleg. 16).

According to the Japanese team, volitional law and natural law "often overlap and penetrate each other" (p. 53). Many writers take Grotius at his word and separate natural law from volitional law (divine and human) (*JBP*, proleg. 1), as if Grotius were eclectically naturalist and positivist. Insofar as all law derives its force from human nature, it hardly seems "a methodological weakness" (p. 43) for Grotius to have used the same inductive method to determine the content of natural law and volitional law. On the evidence, human nature is exceedingly plastic.

Starting with the necessity of self-preservation, an otherwise plastic human nature yields a hierarchy (though hardly a system) of law (pp. 51–55). First is mandatory natural law, then a permissive domain of natural law, itself possessing four levels of flexibility that make it effectively indistinguishable from volitional law (pp. 39–41). The latter includes municipal law, some rules of which are common to many peoples (*jus gentium*), and law "that in the strict sense concerns the mutual society among nations" (*mutua gentium inter se societas*) (p. 54). Finally, there is what today we might call "soft law" in the form of precepts advising humanity and moderation. As several team members intimate, Grotius had no independent, "Grotian" conception of "international society." Given human nature, if there is law among nations, then there is a nominal society among them as well. Grotius canvassed not just the law of nations but law in all its variety, for one purpose:

where there is law, there are limited just causes for war and unlimited opportunities for its mitigation.

Florida International University NICHOLAS ONUF

The Political Psychology of the Gulf War: Leaders, Publics, and the Process of Conflict. Edited by Stanley A. Renshon. Pittsburgh: University of Pittsburgh Press, 1993. 376p. $49.95 cloth, $16.95 paper.

This is an unusually wide-ranging and yet coherent edited book. The distinguished contributors represent a useful diversity of expertise and viewpoints, and they have read and used each other's chapters. The book well meets its two goals of contributing to our comprehension of the causes, unfolding, and consequences of the Gulf War and also to "our theoretical understanding of the psychology of conflict, leadership, decisions and consequences" (p. xviii).

The book is organized in terms of the course of the conflict. Part 1, devoted to the origins of the Gulf War, consists of a chapter by Carol Brown emphasizing Arab myths and mind sets that provided the context for the Gulf War and a brief commentary by Dankwart A. Rustow. Part 2 focuses on leaders and decision making. It includes a chapter on President Bush by Stephen J. Wayne and one about Saddam Hussein by Jerold M. Post, perceptively analyzing their personality characteristics and decision-making methods. Stanley A. Renshon, in a chapter about both leaders, examines what he regards as the good judgment manifest in Bush's decisions and the poor quality of Hussein's. David G. Winter concludes part 2 with a commentary and his analysis of the motive profiles of Bush and Hussein as revealed in their speeches.

Part 2, on the conflict process, includes Janice Gross Stein's detailed analysis of the interaction between the United States and Iraqi governments, explaining why the U.S. government failed to deter Iraq from invading Kuwait and why it failed to compel Iraq to leave before going to war. She concludes, "My analysis suggests that the invasion of Kuwait was probably unstoppable, short of major concessions by Kuwait, . . . and irreversible, short of the use of force to compel complete withdrawal" (p. 144). Jarol B. Manheim provides an informative chapter on efforts to influence foreign policy by the Pentagon, the Bush administration, the Kuwaitis, and the Iraqis, concluding that the Iraqis were not effective and the Kuwaitis were effective in influencing the U.S. agenda. Robert Jervis concludes this part with a commentary relating psychological generalizations with this case.

Part 4, on mass politics, includes Shibley Telhami's insightful analysis of Arab public opinion and variations among different countries and segments of the populations. John Mueller provides a comprehensive, interpretative review of the considerable survey data on American public opinion, and Asher Arian and Carol Gordon do the same for Israeli public opinion. Herbert C. Kelman offers an extended commentary on the impact of the Gulf War and related events on the publics and governments of the region and of the United States. While their impact in the United States appears to be small, they may have much greater consequences in the Arab countries. In this, as in a few other matters, the contributors do not all agree.

Part 5, on the consequences of the conflict, includes Marvin Zonis's contribution on the shattering of the organizing myths of Arab society, Alexander L. George's analysis of the war's possible impact on the international system, and Dankwart A. Rustow's commentary. The book concludes with part 6, an epilogue by Stanley A. Renshon discussing some policy implications.

Although written shortly after the events analyzed, the authors engage important interpretative issues and provide useful information and valuable insights. Assessments of the events' implications for the future are necessarily more tentative. A finding, supported by various strands of evidence reported by several authors, suggests one reason impacts are limited: perceptions of the Gulf War are filtered through preexisting understandings and consequently tend to confirm what people already believe. This sometimes contributes to the polarization of opinions, as in Israel. Furthermore, it is easier to discern how trends already under way are reinforced than to analyze emerging trends that are dampened.

I think that in many ways, this book illustrates the value of an edited book—giving voice to people with different perspectives. If anything, the book would benefit from even more diversity, especially since it stresses the importance of the way different people view events. Nearly all the contributors are North Americans; and the United States is generally viewed from the inside, the Arab countries somewhat from the outside. Shibley Telhami's contribution avoids this, but Arab perspectives on American organizing myths and mind sets would also be helpful.

Although the book is about the political psychology of the Gulf War, what political psychology means is not extensively articulated. The book, in actuality, includes discussions (largely by political scientists) of personality, small-group interactions, peoples' attitudes and opinions, and other subjective factors such as images, myths, religious beliefs, and collective identities. It also includes discussions of social groupings such as social movements. What concerns me is that labeling all that as political psychology may obscure other approaches. For example, shared images, norms, and understandings about roles and structures are analyzed by other social scientists, notably anthropologists and sociologists and they provide alternative ways of looking at these matters.

Finally, although neither the editor nor the contributors present their analyses as an alternative or a necessary complement to the conventional "realist" paradigm, the significance of their contributions would be enhanced if that were explicitly discussed. It seems to me that the findings and interpretations they present reveal a more complex reality than that envisaged in the "realist" model. For example, the great variation in conceptions of national interest over time and among different persons reduces the analytic values of the concept. For another example, the multiplicity of significant actors (human beings all) undermines the notion of the world as a system constituted by states acting as unified, rational actors. But I confess that these matters may be more appropriate for another book. The editor and the contributors to the work under review have produced a fine book, and it is best to do one book at a time.

Syracuse University LOUIS KRIESBERG

The Domestic Bases of Grand Strategy. Edited by Richard Rosecrance and Arthur A. Stein. Ithaca: Cornell University Press, 1993. 230p. $36.50 cloth, $13.95 paper.

Editors Rosecrance and Stein present a focused and disciplined set of empirical critiques of structural realism, and while none of the chapters are blockbusters, they are all good. The book is a useful entry in the literature of theoretical debate, and it is less soporific than the usual exegeses of competing paradigms. Of course, it will not resolve the debate, which has been recycled in various forms for centuries. My own views are eclectic but tilt toward realism.

By demonstrating that domestic politics often affect strategic decisions in ways that do not follow consistently from assessment of international constraints, this collection helps to demonstrate the indeterminacy of structural realist theory. While the authors sometimes refer to realism in general (although some strands of realism accord ample weight to domestic factors), the clear target of the volume is Kenneth Waltz. This is only fair, since Waltz's *Theory of International Politics* (1979) gave it to Rosecrance with both barrels. The authors score a number of hits that should chasten naive hyperrealists who see the distribution of international power as firmly governing state action, although they occasionally overlook aspects of their cases that are consistent with realism.

Michael Doyle points out how some rulers act in their own personal interests, rather than those of their countries; and he debunks the notion that in 1914, socialist internationalism collapsed into nationalism. John Mueller makes the case for ideas, rather than capabilities, as the driving forces in the Cold War conflict. David D'Lugo and Ronald Rogowski look at the effect of "constitutional fitness" on the Anglo–German naval race and conclude that British democracy was better able to extract military capabilities from society. (If they had considered conscription, the comparison might have looked different.)

Arthur Stein sees U.S. grand strategy as underextended before World War II and overextended after it, due to domestic disagreements and legislative–executive conflict. Rosecrance and Zara Steiner contrast the British case before World War II with Stein's analysis, arguing that domestic political pressures overrode both economic constraints and realistic strategic choices. (A realist might counter that some of the difference reflects options conferred by geography, so that Americans could debate *whether* to be involved in the European balance, while the British could only debate *how*.)

Matthew Evangelista invokes "postrealism" as the idea that countries can choose policies to shape their adversaries' internal debates and argues that reciprocity was the logical strategy for the United States to help Soviet moderates disprove the case of Soviet hardliners. Yet he admits that the idea is hard to test, because there was hardly any U.S. reciprocity; and the one slim reed that bears the entire empirical load for the argument (in opposition to the hawkish contention that revving up the arms race compelled Soviet accommodation) is Reagan's agreement to a summit meeting in 1985.

Jack Snyder surveys the interaction between domestic and international sources of nationalism and argues convincingly that focusing on either in isolation can yield counterproductive policies. In the most pungent of the chapters, Chalmers Johnson presents Japan's behavior both before and after World War II as antithetical to realist logic, and discusses the Meiji–Bismarckian model of development as an alternative to Western liberal or Leninist–Stalinist models. He then rails against the ethnocentric economism of Western social science, which he sees as unconscious ideology blinding Americans to Japan's current grand strategy, which is to build a new Asian coprosperity sphere.

Illustrating the variety of ways in which governments can interpret their strategic options is a reminder to the most simple-minded of realists that there is seldom only one rational option dictated by the international system (or that when there is, it is seldom obvious to all in advance) and that adaptation to external imperatives involves political competition among views of what is rational. Contrary to occasional charges in the book, however, structural realism does not deny this, since Waltz argues that it is a theory not of foreign policy choice but of international political outcomes over time. (It is often forgotten that Waltz himself wrote a whole book on domestic determinants, *Foreign Policy and Democratic Politics* [1967].) Yet the confusion is understandable, because there has always been ambivalence in realism about whether its tenets are primarily empirical or normative.

It is not hard to show realism's limits as a predictor. As Matthew Evangelista notes in his chapter, "Defenders of realism might argue that the theory is intended to be probabilistic rather than determinative. . . . Nevertheless, when the range of possible outcomes extends from appeasement to preventive war, one is justified in asking whether alternative approaches might do better" (p. 167). (It is not necessary to dip below the systemic level to show limitations of the theory: while realists all agree that the distribution of power is the most important concern, there is no consensus among them about *which* distribution—multipolarity, bipolarity, or unipolarity—is preferable.)

It is possible to appreciate these limitations without reaching as far as Rosecrance and Stein occasionally do. For example, they criticize U.S. strategists for ignoring nonmilitary dimensions of grand strategy and for failing to ask whether nuclear deterrence was affordable: "Both nuclear strategists and realists tended to neglect patterns of domestic support and economic strength that might affect long term commitment" (p. 4). Yet the most ardent proponents of reliance on nuclear deterrence (e.g., Eisenhower) were economizers worried about the unsustainable expense of large conventional forces. Neorealists like Waltz, Posen, and Mearsheimer opposed the defense spending binge of the Reagan era. And many academic strategists at least partly realist in orientation (e.g., Samuel Huntington, Paul Hammond, Warner Schilling, Glenn Snyder, Robert Art) focused much of their work on internal politics. It is also a stretch when the editors say, "There is no realist strategy to deal with domestic social forces in other countries" (p. 16). How about Henry Kissinger's admiring discussion in *A World Restored* (1957) of how Metternich and his counterparts fostered the consensus on "legitimacy" in the post-Napoleonic era?

At least two simple insights keep realism relevant despite such useful critiques as the volume under review. One, resisted in some respects by Grotians or Wilsonians, is that norms cannot override power when interests conflict. The other is that international politics is marked more by tragic continuities than by progress;

or, as Robert Gilpin put it, "Realism must be seen as a philosophical disposition and set of assumptions about the world rather than as in a strict sense a 'scientific' theory, . . . as an attitude regarding the human condition" (in Robert Keohane's edited *Neorealism and Its Critics* [1986], p. 304). As I suspect at least some of the contributing authors believe, *The Domestic Politics of Grand Strategy* may best be taken as a corrective complement to realism, rather than a refutation of it.

Columbia University RICHARD K. BETTS

China, the United States, and the Soviet Union: Tripolarity and Policymaking in the Cold War. Edited by Robert S. Ross. Armonk, NY: Sharpe, 1993. 224p. $55.00 cloth, $18.95 paper.

There is no consensus among students of international relations on the efficacy of the strategic triangle as an analytical tool for studying the interactions among China, the Soviet Union, and the United States. By contrast, the six contributors to this volume adhere—albeit to varying degrees—to the view that the strategic triangle *is* a useful concept. Nonetheless, as Chi Su's insightful chapter notes, the strategic triangle suffers from three major drawbacks as an analytical concept. First, it overestimates the weight and range of China's power. Although the United States tended to treat the People's Republic of China (PRC) as the superpower that some leading American policymakers thought it might become, the disparity between China and the superpowers had a marked effect on the interactions between the PRC and the Soviet Union.

A second problem with the strategic triangle is that while it depicts the general contour of the *forest* of U.S.-Soviet-Chinese relations, the triangular formation leaves much to be desired when it comes to explaining the trees. For example, in the 1970s, when the Soviet Union was perceived to be on the offensive and U.S.-Soviet relations were adversarial, the concept of triangular relations suggested that China join hands with the United States—and it did in fact move to do so. Similarly, when the Soviet threat began to subside and Beijing began to object strenuously to the U.S. attitude toward Taiwan, the triangular perspective would predict that China would strike a more independent posture. This, too, occurred. But the deficiencies of the triangular paradigm become obvious when one asks more focused questions. Why did the Chinese not attempt to forge a formal alliance with Washington during the 1970s, when Moscow posed such a serious threat, as they had done in the 1950s *with* Moscow and *against* a perceived threat from Washington? Also, why did China take the initiative in offering to normalize relations with the Soviet Union in April 1979, when Soviet expansionism was still at its peak? Why did Beijing not lean more toward Moscow while Gorbachev, and then Yeltsin, were defensive and beset with domestic problems and the United States was not only strong but also allegedly attempting to subvert the Chinese government through a "peaceful evolution" plot? These examples show that there are distinct limits to relying on the concept of the strategic triangle to explain developments in Chinese policy toward the Soviet Union.

Third, the strategic triangle tends to overemphasize the strategic dimension of the triangular relationship at the expense of other factors influencing policy. This deficiency appears to be especially conspicuous for Sino-Soviet relations. The Chinese Communist party has a convoluted relationship with the Soviet Union that predates President Richard Nixon's opening to China (and therefore the emergence of the triangle) by five decades. Hence, while Washington may be thinking in strategic terms and Beijing and Moscow may be following policies that accord with strategic rules, the policies of the two may, in reality, be more driven by historical baggage than strategic calculations. Thus, it is necessary to take pretriangle factors into account.

Stephen Sestanovich's chapter on the impact of China on U.S. policy accepts the reality of the strategic triangle but argues that it created very little usable bargaining leverage. However, the triangle did shape the evolving U.S. calculus of the balance of power. The initial opening to China provided a kind of insurance policy that the terms of a U.S.-Soviet détente would be satisfactory from the U.S. point of view. Over time, however, triangular politics created a different dynamic: they helped to build a U.S. consensus in favor of tougher policies toward the Soviet Union. When the Soviet Union attempted to intimidate China, Washington countered against Moscow, thus increasing the Soviet leadership's sense of isolation and overextension in the 1980s. Ultimately, argues Sestanovich, this contributed to the end of the Cold War.

In a further modification of the triangular concept, several authors point out that the personalities of individual leaders had a marked effect on their countries' policies. For example, the shift in American attitudes toward Moscow after 1975 occurred not because the Soviet Union changed but because the U.S. president changed. Similarly, Ronald Reagan's first secretary of state, Alexander Haig, was willing to pay a higher price for U.S.-Chinese cooperation than triangular dynamics suggested; but this was reversed by his successor, George Shultz.

Since the authors are discussing the same events—albeit from different angles of a prism—there are inevitable overlaps of material. There are also slight differences of opinion among the authors. Robert Ross argues that since one of the factors that made strategic considerations triangular (China's strategic importance due to superpower rivalry) no longer exists, the rules of triangular politics no longer apply. By contrast, Robert Legvold points out that each of the three states is too important to the others—even when Russia is but a pale shadow of its predecessor—to assume safely that developments in one bilateral relationship will never contaminate the other bilateral relationship: triangularity continues to exist. If the book can be said to have a general theme, it is that the concept of the strategic triangle is a valid tool of analysis, albeit much modified by considerations of history, individual personalities, domestic political considerations, and the behavior of states not party to the triangle.

University of Miami,
Coral Gables JUNE TEUFEL DREYER

Making War and Waging Peace: Foreign Intervention in Africa. Edited by David R. Smock. Washington: U.S. Institute of Peace Press, 1993. 290p. $19.95 paper.

Funders of policy-relevant research often worry about getting the results to the appropriate people. Left to themselves, academic researchers tend to publish expensive books or scatter obscure articles. The U.S. Institute of Peace, which sponsors a lot of high-quality research, has established its own press to publish and disseminate the results itself.

This book is a collection of such research on intervention in Africa. As such, it is not really a book in the usual sense: it is more like an issue of a good journal (perhaps explaining why there is no index). The quality of the individual pieces is quite high; but they are only tenuously related to one another, they do not present a comprehensive view of the subject, and there is not even the usual figleaf of an editor's conclusion to try to tie them together. Given these limitations, this is a very useful collection that advances our understanding of an important issue, albeit incrementally. Its low price should make it accessible to both policymakers and academics and encourage classroom adoption.

Overall, the book is stronger on cases than theory. Strong chapters on civil wars in Ethiopia, Sudan, Namibia, Angola, and Mozambique are well done and remarkably current, the best I have seen in print. Three broader chapters examine French, British, and Belgian military involvement; Organization for African Unity intervention; and the notion that successful intervention may involve many outside actors (rather than only one). The final chapter is a summary of Chester Crocker's congressional testimony on improving African peacemaking capabilities.

Overall, the book contributes to the literature both by adding new and important cases and by implicitly confirming some widely held ideas. Christopher Mitchell's argument that different interveners can play different roles in the same case is confirmed both by his own analysis of the two Sudanese cases and by the Mozambique chapter (although the apparent assumption of a single kind of settlement process remains controversial). Several cases support the argument that settlement requires that both sides be strong internally so that they can control their extremists and deliver on their promises. Crocker argues that settlements are the result of "the right alignment—the proper constellation—of local, regional, and international events." Since this cannot readily be predicted, outside mediators should keep trying all the time. (The counterargument that some mediation acts may actually make things worse is not analyzed, although Carter's 1989 failure in the Sudanese case is cited as an example [pp. 126–27].) Finally, the authors are unanimous that foreign intervention does not make war or peace but, instead, strengthens or weakens particular locals and provides new alternatives and options for them.

The book accurately reflects current professional research in this field. Its authors focus on settlement, on encouraging local elites to decide to stop fighting, and on how foreign intervention affects that goal. There is no analysis of the problems of followers (either fighters or noncombatants) and how these may influence collective behavior, including obedience to elites. After civil war, most countries are left with massive problems, such as demobilization, development, and nation building. The role of foreign intervention in such activities is mentioned but ignored—although, presumably, a government's response to these problems will influence whether the settlement "sticks" or not. (This omission is particularly annoying in the Angolan case, where the collapse was attributed precisely to the failure of demobilization.) It is difficult to develop general ideas about when intervention takes place by focusing exclusively on the targets of such intervention, since cases of nonintervention are left out. Any such analysis would have to focus on the politics of the intervening countries themselves, looking for the dogs that do not bark, the interventions that do not occur.

On balance, then, this is a very useful collection, with the virtues (and some of the limitations) of current research on the study of foreign intervention in the Third World.

Rutgers University ROY LICKLIDER

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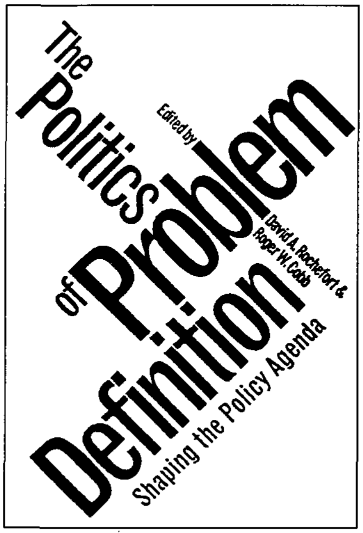
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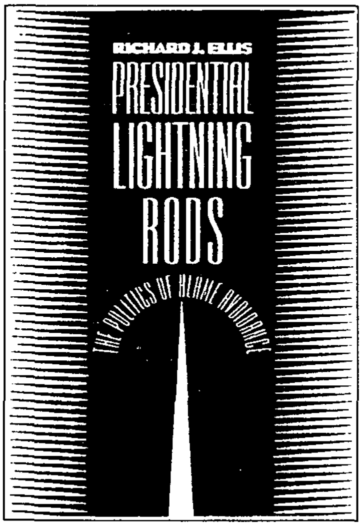

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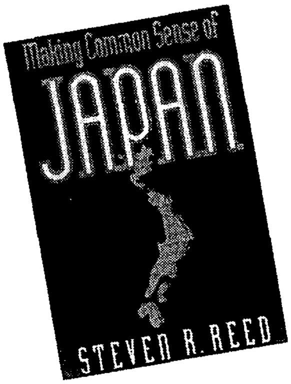

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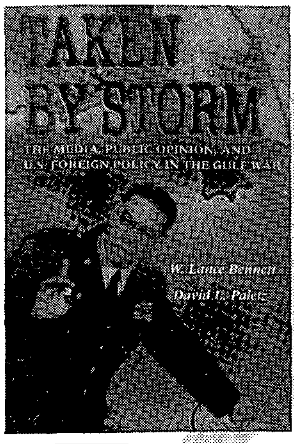
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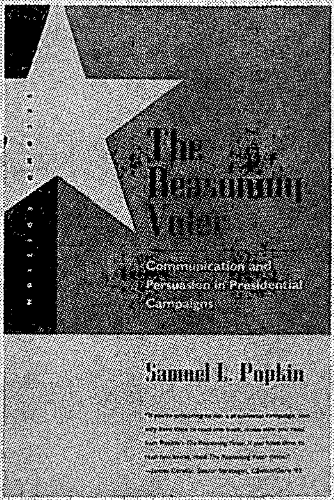

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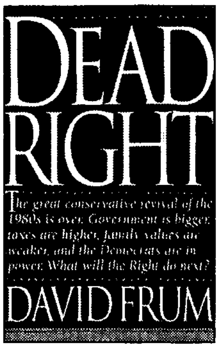
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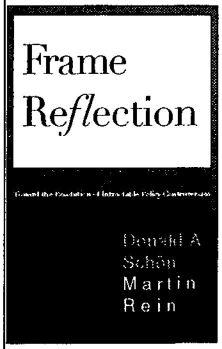
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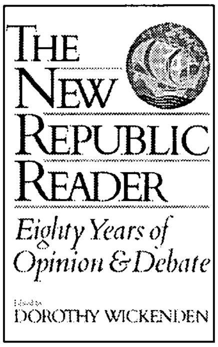
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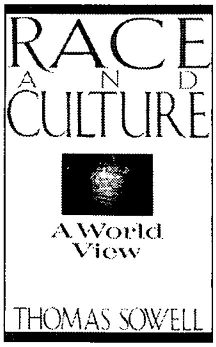
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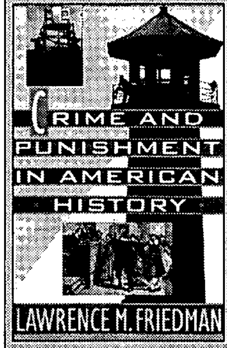
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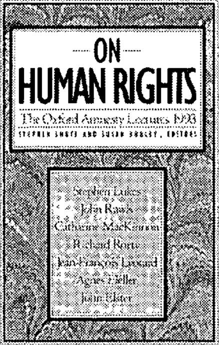
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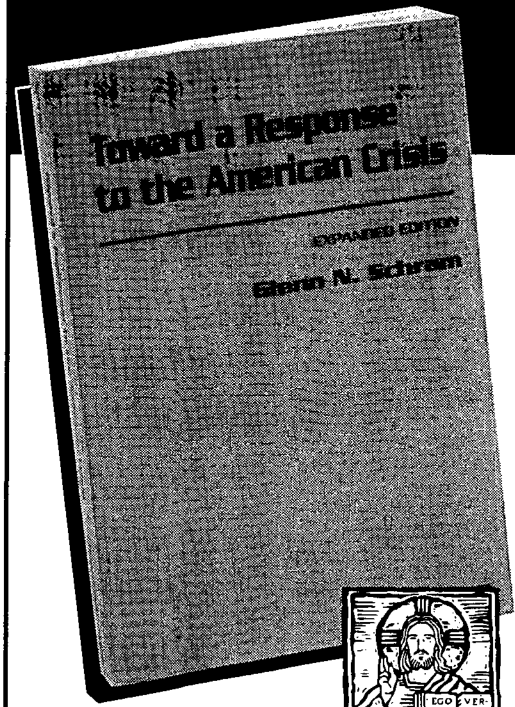

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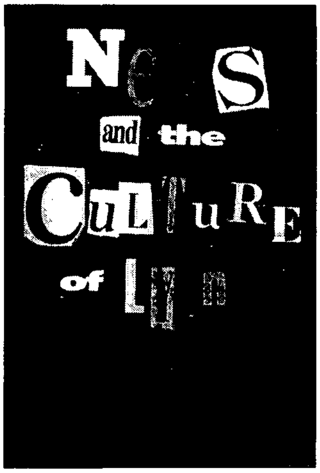

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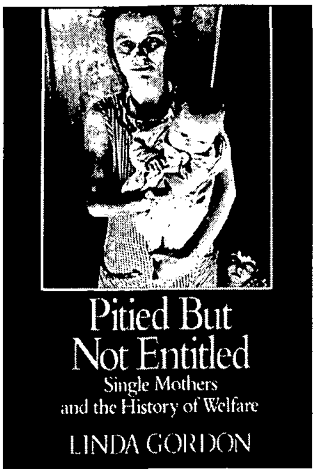

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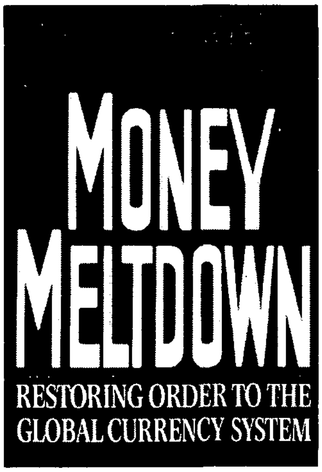

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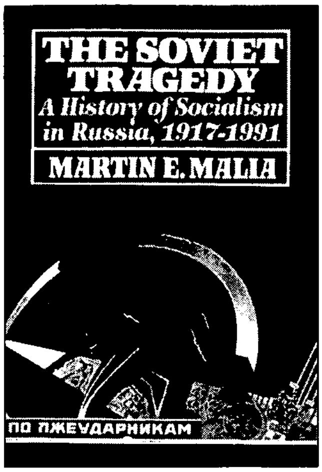

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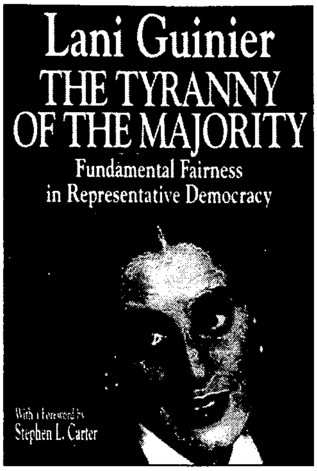
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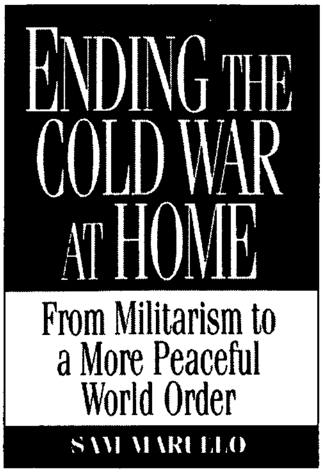
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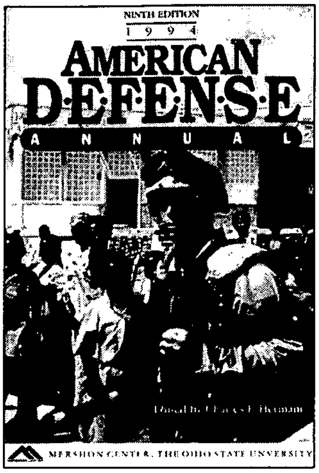

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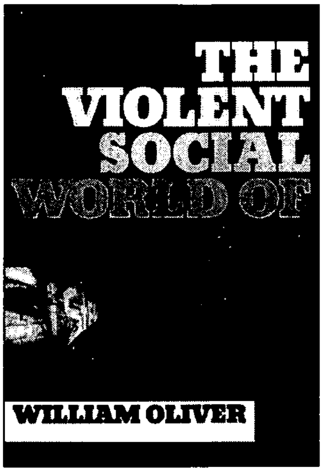

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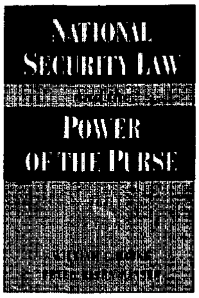

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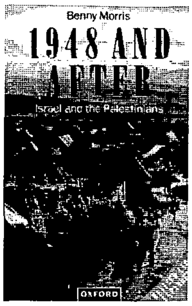

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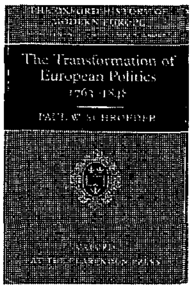
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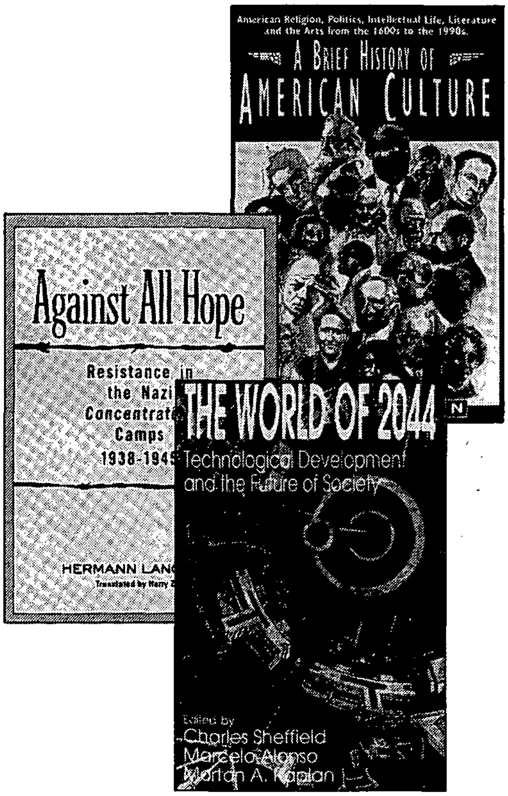

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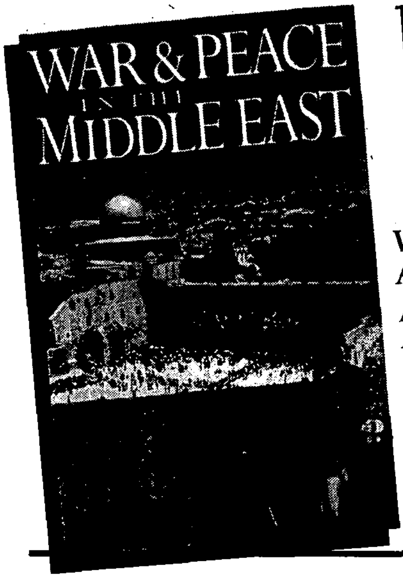

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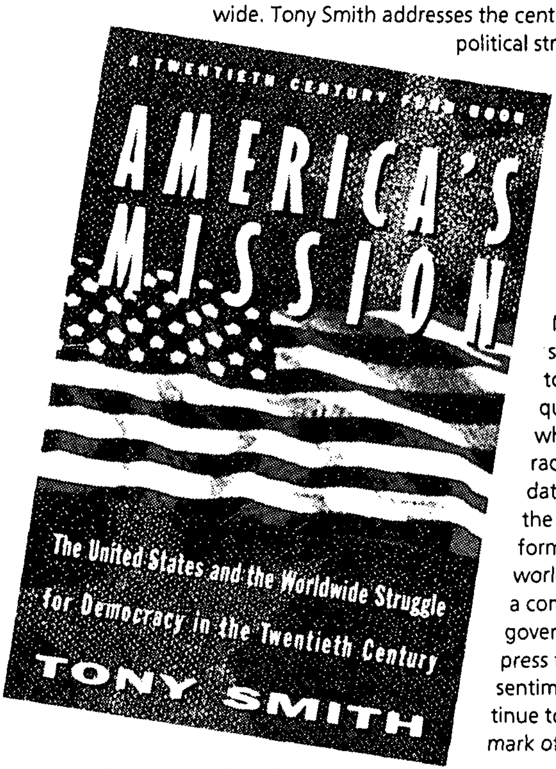
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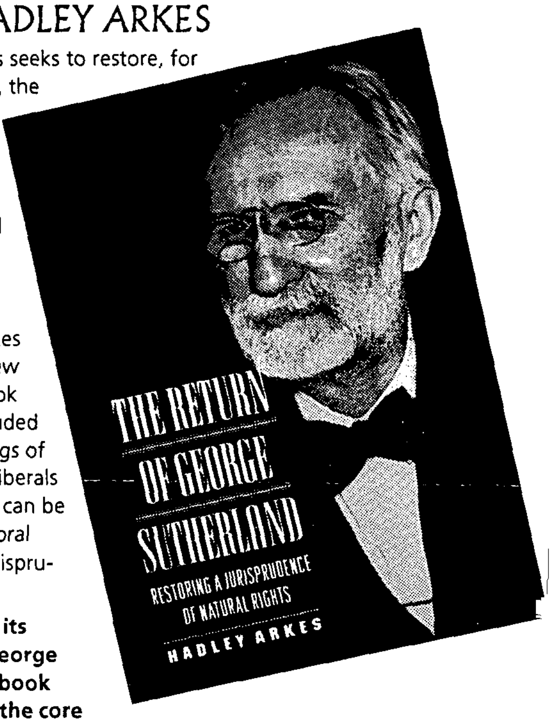
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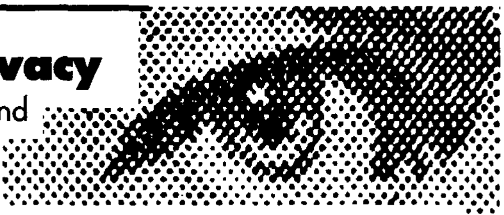

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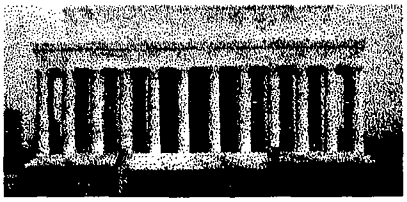

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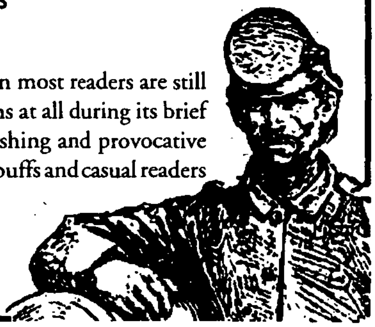

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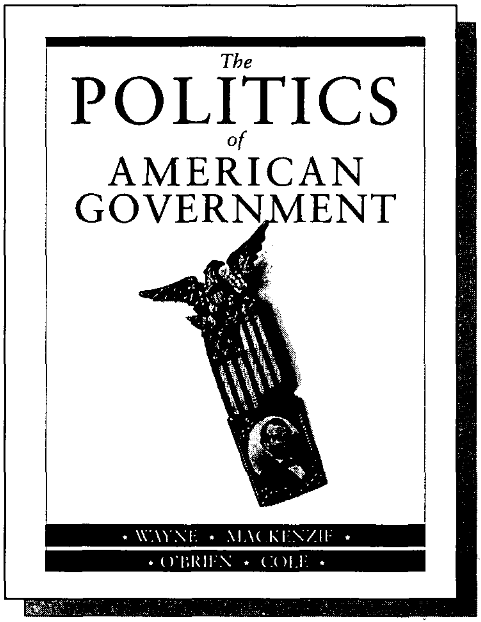

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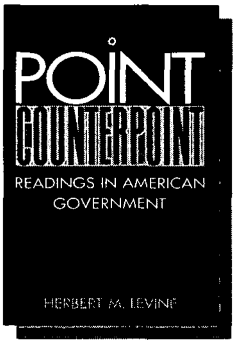

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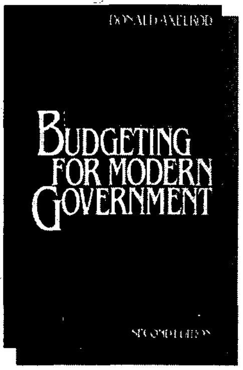

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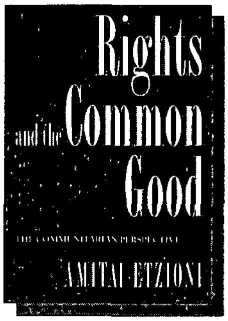

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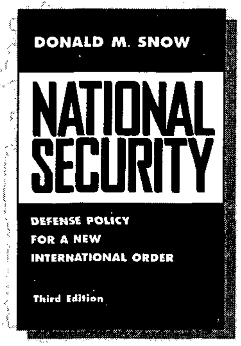

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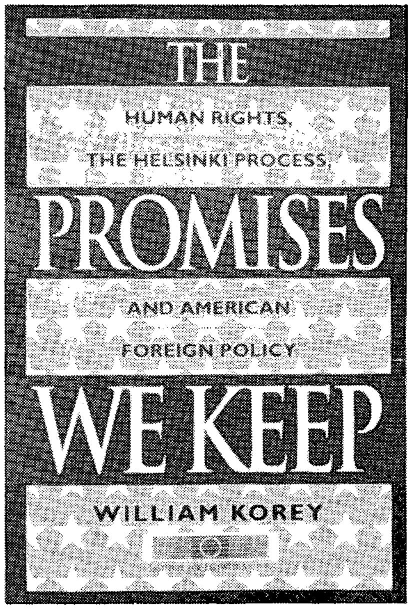

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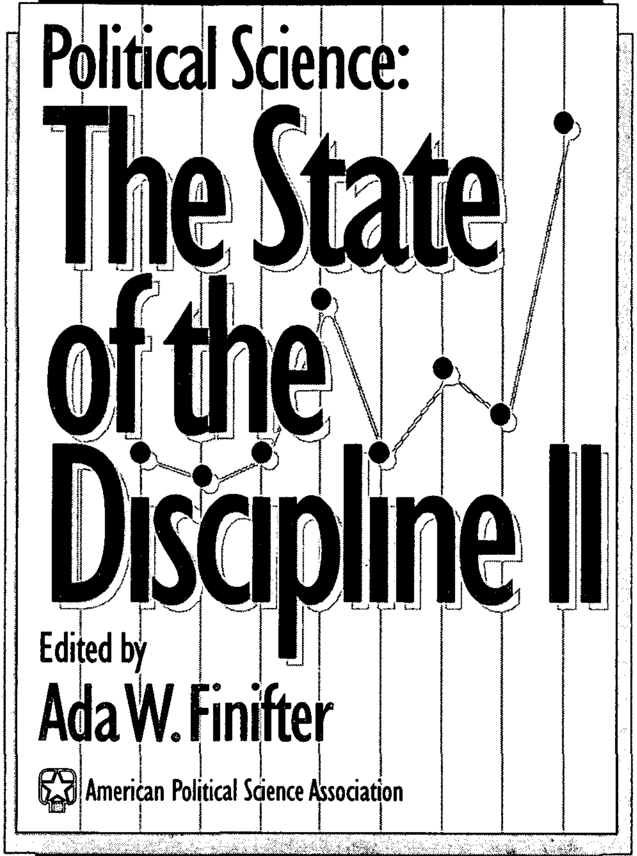
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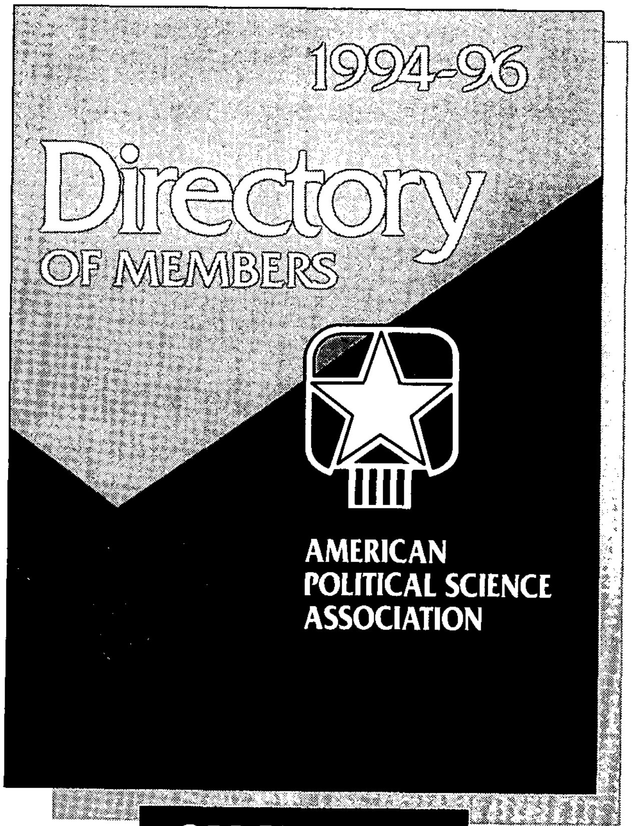

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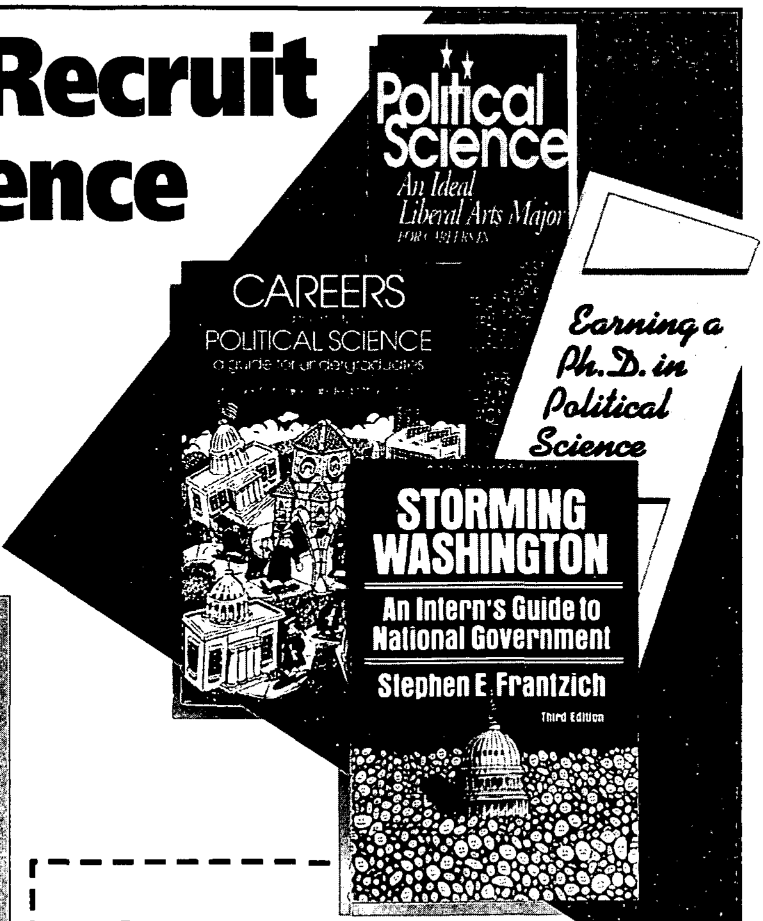
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Table of Contents

ARTICLES

Issues, Candidate Image, and Priming: The Use of Private Polls in Kennedy's 1960 Presidential Campaign
Lawrence R. Jacobs and Robert Y. Shapiro 527

Enhancing Democracy through Legislative Redistricting
Andrew Gelman and Gary King 541

Power and Influence in State Legislative Policymaking: The Interaction of Gender and Position in Committee Hearing Debates
Lyn Kathlene 560

Domestic Political Audiences and the Escalation of International Disputes
James D. Fearon 577

Peace, War, and the Balance of Power
R. Harrison Wagner 593

Strategic Voting Equilibria under the Single Nontransferable Vote
Gary W. Cox 608

The Tower of Babel as a Coordination Game: Political Linguistics in Ghana
David D. Laitin 622

Civic Culture and Democracy: The Question of Causal Relationships
Edward N. Muller and Mitchell A. Seligson 635

On the Idea of the Moral Economy
William James Booth 653

The Piety of Thought in Plato's *Republic*, Book I
Darrell Dobbs 668

Thoughtless Assertion and Political Deliberation
Melissa A. Orlie 684

RESEARCH NOTE

An Individual-level Multiequation Model of Expenditure Effects in Contested House Elections
Christopher Kenny and Michael McBurnett 699

CONTROVERSY

Popular Influence on Supreme Court Decisions
Helmut Norpoth and Jeffrey A. Segal;
William Mishler and Reginald S. Sheehan 711

NOTES FROM THE MANAGING EDITOR 725

BOOK REVIEWS

Political Theory 732
American Politics 751
Comparative Poltiics 776
International Relations 793